SCIENCE

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*MEASUREMENT AND CALCULATION.**

IN my address of a year ago I sought, in a summary way, and by concrete illustration, to indicate how science originates in and advances with observation and experiment. I would now invite your attention to a similar consideration of the rôle which measurement and calculation play in the higher developments of science.

All sciences are at first qualitative. They pass in their growth from the fact-gathering stage of unrelated qualities to the orderly stage of related qualities and thence upward to the stage of quantitative correlation under theory. Such, at any rate, has been the course of all sciences hitherto developed, and it seems safe to predict that such will be the course of those which may arise in the future. The recognition of this fact is of prime importance. It helps us to understand the great relative diversity in perfection among the sciences; it affords a basis for rational optimism with respect to the continued progress of science; and it ought to make the specialists of the older sciences less contemptuous than they sometimes are in their attitude toward the newer ones which have not yet passed the 'rock-naming and bug-hunting stage.'

Whenever a quantitative relation be-

* Address of the retiring President of the New York Academy of Sciences, read February 24, 1902.

tween the factors of phenomena is observed, then measurements may be made in response to the question, What is the magnitude of the relation, if constant, or what are the extent and law of variation of the relation if it is not constant? When the law of relation is known, related quantities are subject to calculation, the measured values of some of them sufficing, through computation, to give the values of the others. All calculations, therefore, presuppose a knowledge of the laws of connection of related quantities, or quantitative theories of the phenomena considered.

Measurements and calculations are of all grades of definiteness, ranging from the smallest probabilities of the doctrine of chances up to the rigorous certainties of mathematical deduction. Thus the degree of precision attainable in the measured and computed quantities of a science is commonly taken as a gauge of its perfection. But it would be a mistake to infer complete perfection from the precision attainable in one or more branches of science. Astronomy, for example, is a marvelously perfect science in certain of its branches, but nevertheless some of its fundamental constants, notably the gravitation constant and the aberration constant, are known with only a low degree of precision.* Whether any quantity may be measured or calculated with precision depends, in general, on the degree of complication of its connections with other quantities, and on the applicability of methods already applied in the determination of other quantities. Frequently, a quantity may be measured directly; but it oftener happens, either by reason of the inapplicability or of the disadvantage of a direct method, that resort is had to an indirect method.

It is a remarkable fact, illustrating the essential unity which pervades the apparent diversity of nature, that all of the numerous quantities with which physical science has to deal may be expressed in terms of a certain very limited number of arbitrarily chosen quantities, or units. The units most commonly used, and those which seem best suited to the present requirements of science, are the units of length, mass and time. All other quantities, however complex, may be expressed readily in terms of these arbitrarily as-

($L^{+3}M^{-1}T^{-2}$) if L, M, T denote the units of length, mass and time respectively.

It should be remarked also that the above expression of Newton's law of gravitation lacks the precision essential for mathematical calculations. To make the statement definite and general m_1 and m_2 must be regarded as infinitesimals, so that the resultant attraction between two finite bodies requires, in general, a summation, or integration, for its exact expression. A widespread error exists in the notion that the above equation is exact if the distance s is the distance between the centers of gravity of the masses. This is true, indeed, for the class of bodies called centrobaric, like homegeneous spheres; but masses in general are not centrobaric.

The gravitation constant is, in C. G. S. units, about 667×10^{-12}, with some uncertainty in the last significant figure.

The aberration constant, which is (if it is nothing more than a kinematical quantity) the ratio of the velocity of the earth in its orbit to the velocity of light multiplied by the number of seconds in a radian, is about 20.5″ with some uncertainty in the next significant figure.

* The gravitation constant is the factor by which the product of two masses divided by the square of their distance asunder must be multiplied in order to express the force exerted by those masses on one another. Thus, if m_1 and m_2 denote two masses, s their distance asunder, F the force of attraction between them, and k the gravitation constant, then

$$F = k \frac{m_1 m_2}{s^2}.$$

It should be remarked that k is not a mere numeral, as many eminent writers on the law of gravitation would seem to imply, but that it is the cube of a distance divided by the product of a mass and the squares of a time; or that its dimensions are shown by the exponents in

sumed fundamental quantities. It is by no means certain, however, that these units will best satisfy the requirements of science in the future. On the contrary, it seems rather probable that advancing knowledge will find some other system of units preferable, if it does not find several different though interconvertible, systems essential. We have, in fact, already attained two such diverse systems in the units of electro-magnetic science.

The study of such systems by the aid of the theory of dimensions, which shows algebraically how the assumed units enter into more complex quantities, is very instructive, not only to the mathematical physicist, but to the general student of physical science.* To illustrate this idea by some simple examples, it is well known that all quantities used in rational mechanics are commonly expressed in terms of length, mass and time. But these quantities might be expressed equally well, so far as algebraical statement is concerned, in many other ways. Thus, we might take energy as one of the fundamental quantities instead of either length, mass or time; in which case our mechanical quantities would be expressed in terms of energy, length and mass; or of energy, length and time; or of energy, mass and time. A consideration of these simple systems shows us, among other things, that rational mechanics might have been developed along lines of thought very different from the lines followed by our predecessors; and the fact that we do not visualize equally clearly all these systems shows that the experience of humanity with physical phenomena has been extremely limited. Most curious and instructive are the system in which length does not appear explicitly and the system in which time does not appear explicitly. May we not see in these systems opportunities respectively for the development of those individuals of our race who seem to possess no realization of distance or no conception of time?

Confining attention to the simpler and more familiar units of length, mass and time, and to a few of the more complex quantities expressed thereby, let us first consider briefly the present status of these fundamental units and the possibility of maintaining their invariability. The stand-

* Designating the units of energy, length, mass and time by E, L, M, T respectively, the dimensions of some of the most frequently used quantities in mechanics are shown in the following tables. In the first of these length, mass and time appear explicitly; in the second, length does not appear explicitly; and in the last, time does not appear explicitly. A glance at the exponents (dimensions) of the symbols shows clearly how definite the meanings of the terms force, energy, power, etc., may be in comparison with the utter ambiguity attaching to them in common parlance.

TABLE I.

Quantity.	Length Factor.	Mass Factor.	Time Factor.
Velocity,	L^{+1}	M^{0}	T^{-1}
Acceleration,	L^{+1}	M^{0}	T^{-2}
Force,	L^{+1}	M^{+1}	T^{-2}
Momentum,	L^{+1}	M^{+1}	T^{-1}
Energy,	L^{+2}	M^{+1}	T^{-2}
Power,	L^{+2}	M^{+1}	T^{-3}

TABLE II.

Quantity.	Energy Factor.	Mass Factor.	Time Factor.
Velocity,	$E^{+\frac{1}{2}}$	$M^{-\frac{1}{2}}$	T^{0}
Acceleration,	$E^{+\frac{1}{2}}$	$M^{-\frac{1}{2}}$	T^{-1}
Force,	$E^{+\frac{1}{2}}$	$M^{+\frac{1}{2}}$	T^{-1}
Momentum,	$E^{+\frac{1}{2}}$	$M^{+\frac{1}{2}}$	T^{0}
Energy,	E^{+1}	M^{0}	T^{0}
Power,	E^{+1}	M^{0}	T^{-1}

TABLE III.

Quantity.	Energy Factor.	Length Factor.	Mass Factor.
Velocity,	$E^{+\frac{1}{2}}$	L^{0}	$M^{-\frac{1}{2}}$
Acceleration,	E^{+1}	L^{-1}	M^{-1}
Force,	E^{+1}	L^{-1}	M^{0}
Momentum,	$E^{+\frac{1}{2}}$	L^{0}	$M^{+\frac{1}{2}}$
Energy,	E^{+1}	L^{0}	M^{0}
Power,	$E^{+\frac{3}{2}}$	L^{-1}	$M^{-\frac{1}{2}}$

ards of length and mass which are now universally adopted in science are the meter and the kilogram respectively, carefully intercompared copies, or 'prototypes,' of which have been distributed by the international bureau of standards to the nations contributing to the cost thereof. The United States possesses two copies of each of these prototypes, and they are, as a matter of fact, our effective working standards, even for the production of standard yards and pounds. It is to be hoped, therefore, that the end of the barbaric system of 'weights and measures' we have inherited from an unscientific ancestry is near at hand, and this not so much in the interest of men of science as in the interests of those less well fitted to struggle with the ingenious intricacies of the British system.

These prototype meters and kilograms are known in terms of the adopted standards, and hence in terms of one another, with a degree of precision which verges close to the limits of the constancy of matter itself. Thus the lengths of the meters are known with an uncertainty expressed by a probable error of only one part in five millions. This degree of refinement corresponds to about one hundredth of an inch in a mile, or to about nineteen miles in the mean distance of the earth from the sun. But this admirable precision is greatly surpassed by that of the kilograms, whose uncertainty falls to one part in five hundred millions. It is well known, of course, that the operation of weighing by means of the balance secures a precision superior to that of every other species of physical measurement; but it is not easy to visualize directly the five-hundred-millionth part of a kilogram. One may get a tolerably definite idea of this magnitude, however, by observing that with the degree of precision in question it would be essential in comparing two kilogram masses to keep the pans of the balance closely at the same level, for a centimeter difference in their altitudes would be appreciable by reason of the variation of the attraction of the earth with distance from its center.*

For present purposes, therefore, our standards of length and mass leave little, if anything, to be desired. But it is a matter of great importance to the future progress of science that these standards be preserved for an indefinitely long period; and although such a contingency seems remote enough now, one can hardly suppress the query as to what would happen to us if our standards should be lost, or if they should unexpectedly prove unstable with the lapse of time. It is quite certain that our standard of length could be recovered with a high degree of precision if such a calamity should befall us during the next ten thousand, or possibly during the next hundred thousand years. Numercus bars of other metals than the alloy used in the construction of the prototype meters are known in terms of the latter. Many base lines scattered at widely separated points of the earth's surface are also known in terms of the meter with a precision of about one part in a million; and although the foundations of the earth are far from stable, we can hardly expect such lines to become systematically shorter

* Denoting the mass of a kilogram by m_1 and the mass of the earth by m_2, the weight of m_1 by w, and the distance from the balance to the earth's center by s (since the earth is nearly centrobaric), the Newtonian law gives

$$w = k\frac{m_1 m_2}{s^2},$$

whence the relation of a small change Δw in w to the corresponding change Δs in s is expressed by

$$\frac{\Delta w}{w} = \mp 2\frac{\Delta s}{s}.$$

Since $\Delta w/w$ is here 1/500,000,000, and since s is about 630,000,000 centimeters, $\Delta s = \mp 0.63$ centimeter.

or longer in so brief a terrestrial interval as a million years. Better still, probably, is the check on the invariability of the meter afforded by Professor Michelson's measurement of it in terms of the wave lengths of particular rays emitted by the metal cadmium.* In this, apparently, we have a cosmic standard, although it remains to be proved that the wave lengths used will remain invariable in the unexplored parts of the universe into which we are journeying along with the solar system at the rate of some kilometers per second.

Our standard of mass is likewise connected directly with various masses which may serve as checks on its stability, and indirectly with the masses of definite volumes of many substances. It is especially well known in terms of the mass of a cubic decimeter of water at a standard temperature. It is less definitely known in terms of the atomic masses of the so-called elements, and it is roughly known in terms of the enormous though slowly varying mass of the earth.† But on the whole, our standard of mass must be regarded as less secure than our standard of length, although the prototype kilograms are less likely to change in mass with the lapse of time than the prototype meters are to change in length; for while such a general variation in volume as is known to occur in metals, especially alloys, need not affect the former, it would almost certainly affect the latter.

Our unit of time is also known with a definiteness that meets in most cases the highest demands of science at the present epoch. The period of rotation of the earth, or the sidereal day, is the standard interval of time, though it has been found convenient for many purposes to use the shorter interval of a mean solar second, of which there are 86,164.1 in a sidereal day. That the earth rotates with wonderful regularity is a fact of the highest importance to science. Without that regularity the development of sidereal and planetary astronomy, with all they have entailed, would have been impossible except by the discovery of some other equally trustworthy timekeeper. But the laws of mechanics, which show us plainly why the earth rotates with such remarkable regularity, also show us that its period of rotation is subject to sources of disturbance, some tending to increase and some tending to decrease that period, whose effects,

* See Tome XI., *Travaux et Mémoires du Bureau International des Poids et Mesures*, Paris, 1895. It is remarkable that the ratios of the three wave lengths used to the meter were measured with a precision requiring seven significant figures, the uncertainty amounting to a few units only in the last figure. Thus the values of the wave-lengths used (designated as red, green and blue respectively) are as follows, in microns, or millionths of a meter:

0.643,847,2,
0.508,582,4,
0.479,991,1.

† If we could measure the gravitation constant with a precision extending to five significant figures, the mass of the earth would at once become known to the same degree of precision, provided only that the law of gravitation is exact to the same number of figures. For I have shown that the product of that constant and the mean density of the earth is known with a precision expressed by five significant figures. Thus, calling the gravitation constant k and the mean density of the earth ρ,

$$k\rho = 36797 \times 10^{-11} / (\text{second})^2$$

This relation may be otherwise expressed by the following theorem: Let τ be the periodic time of an infinitesimal satellite which would revolve about the earth close to the equator (assuming no atmospheric resistance). Then the theorem asserts that

$$k\rho\tau^2 = 3\pi$$

where π is the ratio of the circumference to the diameter of a circle. The value of τ is 1 hour, 24 minutes, and 20.9 seconds. See *Astronomical Journal*, Vol. XVIII., No. 16.

though too minute to be appreciable in such intervals as are known to human history, must certainly become considerable in the course of terrestrial history. Thus, the contraction of the earth due to secular loss of heat tends to shorten the day, while accumulations of meteoric dust and tidal friction tend to lengthen it.* There exists also a graver source of disturbance in the slow rising and sinking of the crust of the earth in different latitudes so often pointed out by geologists. Such movements are only partly compensating in their effects on the day, and it seems highly probable that they may cause irregularities amounting to a few seconds in a century without entailing any noteworthy fluctuations of the relative positions of the land and sea.†

It appears, then, that our time unit is the least stable of the three fundamental units and hence the most in need of checks on its stability. Various other standards of time have been proposed, but none of them meets the requisites of permanency and availability. The interests of astronomical science especially demand that efforts be made to find in the solar system some better timekeeper than the earth. Possibly the fifth satellite of Jupiter may serve as a control on the constancy of rotation of the earth.

Turning now to a consideration of the more complex quantities which are expressed in terms of length, mass and time, we enter the boundless fields of physical science in which measurement and calculation have revealed to us all ranges of magnitudes from the vanishingly small to the indefinitely large. It is in these fields that we learn something definite concerning the limitations of our senses; for while measurements alone carry us but a little way along lines of research, calculation discloses not only the unseen, but also, in many cases, phenomena which are quite beyond the reach of any direct sense perception.*

To begin with quantities near the lower limit of determination, think, for a moment, what is going on in the air which for the present is the main medium of communication between us. No one has ever seen the particles of the atmosphere in the sense that we have all seen the particles, or corpuscles, of the blood. But we probably know more about the molecules of gases than we do about blood corpuscles. By actual count it is known that there are four to six millions of the latter in a cubic millimeter; and with equal definiteness calculation shows us that there are about a million million million molecules in a cubic

* I have discussed the effects of secular cooling and meteoric dust on the length of the day in a paper published in the *Astronomical Journal*, Vol. XXI., No. 22, July, 1901. From this paper it appears that the change in length of the day from secular cooling cannot be perceptible during any such brief interval as that of human history (twenty centuries, say); but that in the course of complete cooling, or in a million million years, say, the change in length of the day may amount to as much as six per cent. of its original length.

From the same paper it appears that accumulations of meteoric dust will only begin to be perceptible in their effects on the length of the day when the process of secular cooling has been substantially completed. In a subsequent number of the *Astronomical Journal* (Vol. XXII., No. 11), Dr. G. Johnstone Stoney has shown that if the compression produced by a layer of meteoric dust is taken into account the effect will be still less than that just indicated.

† See 'Mathematical and Physical Papers of Lord Kelvin,' Vol. III., pp. 333–335, Cambridge University Press, London, 1890.

* The reader may be referred to a very instructive paper by Dr. G. Johnstone Stoney entitled 'Survey of that Part of the Range of Nature's Operations which Man is Competent to Study.' *Scientific Proceedings of the Royal Dublin Society*, Vol. IX., No. 13; *Philosophical Magazine*, Fifth Series, No. 294, November, 1899; published also in *Report of Smithsonian Institution* for 1899.

millimeter of the air around us. Notwithstanding this apparently crowded assemblage, the individual molecules move about in the liveliest manner, their average speed being about five hundred meters per second, and this in spite of the fact that the average length of an unimpeded journey is barely visible by the aid of the best microscopes. Each molecule must therefore collide with its neighbors astonishingly often, the encounters occurring, in fact, about five thousand million times per second.*

More surprising still than the properties of assemblages of molecules forming gases are the properties of the individual molecules, especially when they are made up of two or more atoms. Such miniature systems, comparable, probably, in complexity with the Martian and Jovian subsystems of the solar system, exhibit degrees of constancy which rival the invariableness of the fixed stars themselves. This is particularly the case with their rates of vibration as disclosed by the spectroscope. These rates afford one of the most delicate tests of the properties of matter, whether it is found on the earth or on the most distant star; and yet the vibrations, which recur with a regularity equal to, if not surpassing, the regularity of the rotation of the earth, are executed at the rate of some hundreds of millions of millions per second.† Herein, perhaps, we may find a cosmic unit of time as well as a cosmic unit of distance, though both appear to be inconveniently small for terrestrial purposes.

But the smaller bodies of the universe do not end with molecules and atoms of gases. Recent investigations point to the conclusion that there is another order of bodies of much smaller dimensions and possessing still more wonderful properties. These have been called corpuscles.* Their density is only about one thousandth as great as that of the lightest gas, hydrogen; they are freely given off by several of the so-called radio-active substances; and they move about with speeds of the same order as the velocity of light. It appears not improbable that they play a most important rôle in cosmic as well as in terrestrial physics, and the amount of attention being given to them justifies the hope that their study may illuminate many obscure corners in the realm of molecular science.

Passing per saltum from the smallest measureable and calculable quantities to those with which we have an every-day familiarity, I would direct your attention to the great number of articles of commerce

* See, for example, 'The Kinetic Theory of Gases,' by Dr. Oskar Emil Meyer, translated by Robert E. Baynes, Longmans, Green and Co., New York, 1899.

† The number of vibrations per second corresponding to any given wave-length of light may be easily computed. For the velocity of light is about 300,000 kilometers, or 3×10^{14} microns per second, and this divided by the wave-length in question gives the number of vibrations per second. Thus the average wave-length of the cadmium rays used by Professor Michelson (cited above) is about half a micron. The material sources of these rays must vibrate, therefore, about six hundred million million times per second.

* See a paper by Professor J. J. Thomson, 'On Bodies Smaller than Atoms,' *Popular Science Monthly*, August, 1901.

See also a paper by Professor John Cox on 'Comets' Tails, the Corona and the Aurora Borealis,' *Popular Science Monthly*, January, 1902.

A fact of great interest in connection with the 'corpuscles' considered in these two papers is the repulsion of light impinging on bodies, the amount of which has been actually measured recently by several observers. This repulsion between the sun and the earth is very great, amounting to about a hundred million million dynes; but the gravitational attraction between these bodies is about forty million million times as great as that repulsion.

which are now weighed, measured and rated with precision and sold at a cost which, a half century ago, would have been thought quite impossible. Standard yards, meters, pounds and kilograms, and pocket time-pieces that will run within a few seconds per day, are available at prices within the reach of all who need them. Screws and screw gauges which will easily measure a hundredth of a millimeter (or four ten thousandths of an inch) are articles of trade; beautifully true spheres of steel or bronze may be had for a few cents each; helical springs of the finest steel and of remarkable uniformity are sold for a dollar a dozen; while articles like wire, tubing, sheet metal, and an indefinite variety of tools and machinery are made with a degree of perfection and at a cheapness of cost which would have been regarded as quite unattainable by the founders, for example, of the New York Academy of Sciences. The ready availability of, and the constant demand for, all these products to meet the daily needs of the complex civilization of our time affords a sufficient answer to him who would question the efforts spent in attaining those products or the efforts applied in subjecting new objects of study to the rigorous tests of measurement and calculation.

But the principles of measurement and calculation are not limited in their application to external objects, or to the properties of what we are sometimes pleased to call 'gross matter.' They apply equally aptly in many ways to man himself, and it is clear that with advancing civilization we may confidently expect such application to be greatly extended. While we have not yet attained formulas which will comprehend the vagaries of the individual, we have many formulas which will accurately express the resultant of those vagaries as manifested in racial types. A life insurance company, for example, may not assert at the beginning of a year that any individual of ten thousand men of the same class will die within the year, but it may assert with practical certainty that a definite number of this class will die within the year. Such 'facts and figures' are trite enough, of course, but what we commonly fail to see and appreciate is the solid basis on which they rest, and how greatly it would be to our advantage to extend the same sort of reasoning that has built up great systems of fire and life insurance into other departments of human affairs. Most people, I fear we must infer, are, like Thomas Carlyle, still scoffers at statistics, and few, even of the educated, have any adequate conception of the order which the principles of probability will bring out of the apparent disorder of statistical data.

Of the larger objects of the universe to which measurement and calculation have been applied with success, the earth easily surpasses all others in interest and importance. So great has been this success that one may assert that we know more of the earth than we do of any other body to which science has given attention. Its size, its shape, the amount and arrangement of its mass, its magnetic properties, its speeds of rotation and translation, its precession and nutation, and the lately discovered wabbling of its axis of rotation are all known with a definiteness which is truly surprising when one considers its magnitude and the degree of complexity of those properties. That the eight thousand miles in its diameter should be known within a few hundred feet, that the two hundred millions of square miles in its surface should be known within a few hundred square miles, or that the acceleration of gravity at any point on its surface should be known within a few millimeters per second per second, are results little short of marvelous when one reflects that they have

all been attained within the brief interval of two hundred and fifty years. It would be quite wrong, however, to consider these achievements of geodesy as marvelous from the point of view of science. They are, rather, just such results as persistent scientific investigation has always produced, and such as we may safely predict will be uniformly produced by persistent scientific investigation in the future. The element of the marvelous comes in only when one takes account of the fact that these grand results were attained by a very small number of men, mostly members of academies, struggling, like our own, to maintain an existence, in whose work the general public took little interest, and whose names, even now, are much less known than the names of the obscure philosophers and the obscene poets of antiquity.

Geodesy is undoubtedly the most advanced of the sciences in which measurement and calculation have attained a high order of certainty. It has made modern commerce possible, and it seems destined to play a still more important rôle than it has hitherto in the advancement of terrestrial affairs. It has also made modern astronomy possible, for the certainty of its data enables us to measure not only the dimensions of the solar system, but also the approximate dimensions of the visible universe.

Not less important to the progress of science and to the general advance in human enlightenment are the achievements of the allied science of geology. It cannot boast, as yet, like geodesy, of a high degree of precision in measurement and calculation, for it deals, in general, with phenomena which have not yet been reduced to simple laws. But, on the other hand, its subject-matter is more obvious and tangible, and it appeals therefore more forcibly and continuously to the average mind. No science seems comparable with geology in the completeness with which its history and its main processes are contained in the subjects and objects of investigation. Whoso would read the story of the earth's crust will find it written and illustrated in infinite detail in the rocks themselves. No vivid or perfervid imagination of the historian has concealed the facts or misinterpreted their sequence; they are all recorded with a truthfulness that shames the straightest human testimony and with a permanency which permits comparison and verification in endless repetition.

Geology illustrates more clearly, perhaps, than any other science the value of measurement and calculation when the order only of the quantity sought can be attained. The determination of the fact, for example, that nothing short of a million years is a suitable time unit for measuring the age of the earth, was an achievement whose importance can hardly be overestimated; indeed, our race may yet require decades, if not centuries, to appreciate its full significance, for in spite of the great advances in our times it appears probable that not one in a thousand of the good people with whom we live realizes how profoundly definite acceptance of such a fact must modify thought.

A criticism which the devotees of the so-called humanistic learning often apply to such matters of fact, and which is still occasionally accepted by men of science, helps us to see the absolute need of countless recurrences to the evidence so well exhibited in the crust of the earth. "Ah!" says the humanist, "I observe that the physicists and the geologists do not agree on the age of the earth. Some say it is ten million years, others that it cannot be more than two hundred million years, and others that it cannot be less than a thousand million years. I conclude, therefore, that so long as your doctors disagree in this manner, we may continue to accept the age

recorded in our sacred books." Thus easy is it to mistake the order of a quantity for the quantity itself.

When we pass from terrestrial limitations to celestial phenomena the field for measurement and calculation is immensely enlarged, though the results attainable are less easy of ready appreciation. The Jovian, the Saturnian and the Martian subsystems, which have been pretty thoroughly explored by the observer and the computer, present to us the type, apparently, not only of the solar system, but of the galaxy of systems within telescopic view. And the surveys of the heavens now in progress indicate likewise that isolated stars are the exception rather than the rule, and that the visible stars are generally attended by one or more satellites, which are probably oftener dark than bright bodies. Visual and photographic measurements have, in fact, united in recent years in the demonstration that the number of material bodies in the universe is enormously greater than we have hitherto imagined. Here again, however, as in the case of the geological phenomena just referred to, we must be content to a great extent for the present with a knowledge of the order of the quantities measured and calculated. But to be able to state what is the order of the distances which separate the fixed stars from one another, the order of the volume of the visible universe, the order of the quantity of mass in that volume, and the order of the time unit requisite for the expression of the historical succession of celestial events, seems little short of a stupendous contribution to knowledge when one reflects on the obstacles, material and intellectual, that have stood in the way of its attainment.

The distances asunder of the stars are so great that the hundred and ninety odd millions of miles in the diameter of the earth's orbit about the sun make an inconveniently small base line for the measurement of the least of those distances and a hopelessly inadequate one for the measurement of the greatest of them. It would appear more fitting, in fact, to express such distances indirectly in the number of years it takes light moving at the rate of 300,000 kilometers per second to traverse them. Assuming with Lord Kelvin that the visible universe is comprised within a sphere whose radius is equal to the distance of a star whose parallax is one thousandth of a second, this distance would require light about three thousand years to pass over it, while the average distance asunder of the visible stars is considerably less, but still of the same order. Lord Kelvin has shown also in a profound mathematico-physical investigation recently published* how we may assign limits to the amount of mass in the visible universe. It appears from this investigation that there are something like a thousand million masses of the magnitude of our sun within that universe. The figures for this amount of mass have little meaning to most of us when expressed in ordinary units. The mass of the earth, for example, with its $6{,}000 \times 10^{18}$ metric tons,† is a mere trifle, for the sun has about 327,000 times as much mass as the earth. The mass of the sun therefore is the obviously convenient unit in this case; and we have only to imagine our solar system surrounded by a thousand million such suns, each in all probability attended by a group of planets, to get a sufficiently clear idea of the quantity of mass within visual range of our relatively insignificant

* 'On Ether and Gravitational Matter through Infinite Space,' *Philosophical Magazine*, August, 1901. 'On the Clustering of Gravitational Matter in any Part of the Universe,' *Nature*, Vol. 64, No. 1669.

† The metric ton of 1,000 kilograms, or 2,205 pounds, is about the same as our 'long ton' of 2,240 pounds.

terrestrial abode. And the time scale for the varied events which take place in the interaction of these millions of suns is not less imposing when expressed in familiar terms. A million years is the smallest unit suitable for estimating the history of a star, although the record of that history is transmitted to us through the interstellar medium by vibrations whose period is so brief as to almost escape detection.

Measurements and calculations have thus made known to us a range of phenomena which is limited only by our sense perceptions, sharpened and supplemented by the refinements of mathematical analysis. In space and mass relations these phenomena exhibit all gradations from the indefinitely small to the indefinitely large; and in time they point backward to no epoch which may be called a beginning and forward to no epoch which may be called an end. Dealing chiefly with those aspects of phenomena which possess permanence and continuity, or at least a permanence and a continuity compared with which all human affairs appear ephemeral and fleeting, measurement and calculation tend to raise man above the level of his environment. They bid him look forward as well as backward, and they assure him that in a larger study of the universe lies boundless opportunity for his improvement.

But while that sort of knowledge which has been reduced to quantitative expression has done more, probably, than all else to disclose man's place in and his relations to the rest of the universe, it would appear that mankind makes relatively little use of this knowledge and that we are not yet ready, as a race, to replace the indefinite by the definite even wherein such substitution is clearly practicable. It is a curious and a puzzling, though perfectly obvious, fact that mankind as a whole lives less in the thought of the present than in the thought of the past, and that as a race we have far more respect for the myths of antiquity than we have for the certainties of exact science. Our ships, for example, are navigated with great success by aid of the sextant, the chronometer, and the nautical almanac; but what company would dare set Friday as the day for beginning the transatlantic voyage of a passenger steamer? From time immemorial tradition has dominated reason in the masses of men. Each age has lived, not in the full possession of the best thought available to it, but, rather, under the sway of the thought of some preceding age. We are assured even now, by some eminent minds, that the highest sources of light for us are nearly all found in the distant past; and a few go so far as to assert that modern science is merely furbishing up the half-lost learning of ages long gone by.

The work of academies and other scientific organizations is therefore nowhere near completion. Great strides toward intellectual emancipation have been made during recent times, but they have served only to enlarge the field for, and to increase the need of, that sort of knowledge which is permanent and verifiable. Measurement and calculation have furnished an invaluable fund of such knowledge during the two centuries just past, and we have every reason to anticipate that they will furnish a still more valuable contribution to such knowledge in the centuries to come.

R. S. WOODWARD.
COLUMBIA UNIVERSITY.

'NATURAL HISTORY,' 'ŒCOLOGY' OR 'ETHOLOGY'?

A STUDY of recent literature reveals the fact that zoologists are much in need of a satisfactory technical term for animal behavior and the related subjects which go to make up what is variously known as 'natural history,' 'œcology' and 'biology'

in the restricted German sense. The need is also apparent in recent discussions in SCIENCE. As the number of workers in the field above indicated is rapidly increasing, any attempt to fix the terminology, if at all feasible, is certainly timely. In the opinion of the writer all the terms above mentioned are open to serious objection and should be avoided at least by zoologists who use the English language.

Most objectionable is the term 'natural history' on account of the number of its connotations. Not only may it be understood to include everything from mineralogy to anthropology and ethnology,* but even its more special meanings are most confusing. To convince ourselves of the truth of this statement we need go no further than the writings of Huxley. In his well-known essay 'On the Educational Value of the Natural History Sciences' (1854) and the 'Study of Biology' (1876) he uses the term as synonymous with 'biology.' After tracing the introduction of the word 'biology' to Lamarck and Treviranus† he says (p. 268): "'That is the origin of the term 'biology' and that is how it has come about that all clear thinkers and lovers of consistent nomenclature have substituted for the old confusing name of 'natural history' which has conveyed so many meanings, the term 'biology.'" Nevertheless, in the introduction to his little classic on the crayfish (p. 4) he speaks of "'that accurate, but necessarily incomplete and unmethodized knowledge which is understood by 'natural history.'" To this subject he devotes the opening chapter of the work above mentioned, and it is clear that he uses the term in one or both of two senses: first, to designate an historical or phyletic stage in the development of biological science, and second, as the name of a special discipline, which, though the oldest of all the biological disciplines, still survives and deserves to be cultivated. In view of this multiplicity of meanings, it would certainly be most expedient if we could restrict the term 'natural history' so that it would apply only to certain historical aspects of zoology and botany.

The origin and use of the term 'œcology' are well known. It was first introduced by Haeckel in his 'Generelle Morphologie' (1866, Vol. II., pp. 235, 236) as Professor Bessey has stated* and not as Dr. Bather supposes† in the 'Natürliche Schöpfungsgeschichte,' although a more expanded definition of the term occurs in the various editions of this work and in the 'Anthropogenie.' It should be noted that in the work just mentioned Haeckel distinguished accurately between 'œcology' and 'chorology,' including both, evidently as coordinate subjects under his third ('relational') subdivision of physiology.‡ The term 'œcology,' thus originally proposed by an eminent zoologist, has been adopted by the

* Conf., *e. g.*, Leunis' 'Naturgeschichte' and Woods' 'Natural History of Man.'

† Incidentally it may be remarked that the use of this term to cover both botany and zoology appears to be older than Huxley and other recent writers have supposed. According to Father E. Wasmann S.J. ('Biologie oder Ethologie?' *Biol. Centralbl.*, Bd. 21, No. 12, 1901, p. 392) who can write with authority on this question, the word was used by the schoolmen: "Aristotelian scholastics designated the study of living beings as 'biology.' The 'Biologia inferior' treated of organic human, animal and plant life; the 'Biologia superior' of the psychic life of man and animals," etc.

* SCIENCE, Vol. XV., No. 380, p. 593.

† SCIENCE, Vol. XV., No. 384, p. 748.

‡ Dr. Bather stigmatizes those who use the term 'chorology' as 'pedants,' overlooking the fact that we are not in the habit of applying this name to Haeckel and Huxley, both of whom must have found the word decidedly more concise and euphonious, and therefore better, than 'zoogeography,' 'phytogeography' or even 'geographical distribution.'

botanists, its spelling has been altered, apparently with no other gain than that of saving a letter, or rather part of a letter, and the meaning has often been modified till it is almost equivalent to 'chorology,' or at any rate 'chorological œcology.' And now the zoologists are reappropriating this term, modified spelling, meaning and all, in a manner which reminds one of the case of the good old German word 'Faltstuhl' (Eng. faldstool) which was boggled by the French to 'fauteuil' only to be again reappropriated, with much unction, in its unrecognized form by both English and Germans. It seems to the writer that it would certainly be expedient, not to say generous, for the zoologists to leave the botanists in undisputed possession of the term 'œcology,' especially as they seem to set some store by it. For, in the first place, the term was not a very happy one to begin with, no matter how we interpret the *οἶκος* part of the word. Haeckel intended it to mean something like the economy of nature ('die Lehre vom Naturhaushalte'), but one is at first inclined to understand it as referring merely to the habitat, or even to the dwelling or nest of an organism. This sense, in which it has been understood by Wasmann (*loc. cit.*, p. 392) and many other zoologists, not to mention botanists, is too narrow for the purpose we have in view, as will appear from the sequel.

Ever since the botanists adopted the word 'œcology' and applied it to the important subject which they are exploiting with such zeal and profit, there has been comment to the effect that the zoologists have been unduly neglecting a very promising province of their science. This certainly involves some misconceptions. The zoologists have perhaps distinguished somewhat more rigidly than their botanical brethren between 'chorology' and the proper province of 'œcology,' and in both of these subjects work worthy of the greatest admiration has been accomplished. If we confine our attention to zoological 'œcology' we find that it begins with Aristotle and Pliny, and a rapid survey of recent centuries shows that investigators like the following have devoted whole years of their lives to work in this field: Redi, Swammerdam, Roesel von Rosenhof, Réaumur, Bonnet, Buffon, Trembly, White of Selbourne, Erasmus and Charles Darwin, Wallace, Bates, Belt, Hudson, Romanes, Audubon, Wilson, Coues, Brehm, Houzeau, Leuckart, von Siebold, Semper, Steenstrup, Fritz Müller, Fabre, Francois and Pierre Huber, Giard, Plateau, Adler, Forel, Lord Avebury, Wasmann, Moggridge, McCook, Adlerz, Janet, Marchal, von Buttel-Reepen, Maeterlinck, Riley, Grassi, Lang, Dr. and Mrs. Peckham, Poulton, Silvestri, Erich Haase, Dahl, Escherich, etc. These are but few of the many whose works are scattered through the whole wide range of zoological literature. And there are undoubtedly many others who have investigated subjects like animal migration and the myriad problems suggested by whole groups of animals with which the writer has only a superficial acquaintance. That some botanists, and some zoologists, too, for that matter, have failed to appreciate the importance of the work accomplished by the above-mentioned 'œcologists' is easily explained. One observes that only a small minority of these investigators worked under university auspices. It is only too evident—and only too humiliating—that Schopenhauer's diatribes apply to the zoologists as well as to the metaphysicians, for the investigators above mentioned were *'amateurs'* in the true sense of the word, *i. e.*, lovers of animal life, and most of them therefore lived and worked untrammeled by the interminable 'Rück-

sichten' and 'Nachsichten' of university life. Here one is inclined, with Schopenhauer, to put a higher estimate on their investigations than on many of the publications of academic 'professionals,' especially as the work of the latter is coming to be more and more the expression of ephemeral laboratory fads, inflated with the intellectual infection so inseparable from 'schools' and 'tendencies' of all kinds.

The failure of zoologists to cultivate the province of their science corresponding to the 'œcology' of the botanists is more apparent than real for a second reason; viz., the great complexity of the zoological as compared with the botanical phenomena to be organized and methodized. And this leads us to a further reason for abandoning the term 'œcology' in zoology, and for suggesting the adoption of one essentially different. While botanists and zoologists alike are deeply interested in the same fundamental problem of adaptivity, they differ considerably in their attitude, owing to a difference in the scope of their respective subjects. The botanist is interested in the effects of the living and inorganic environment on organisms which are relatively simple in their responses. The zoologist, however, is more interested in the expressions of a centralized principle represented by the activity of the nervous system or some more general and obscure 'archæus' which regulates growth, regeneration and adaptation, carrying the type onward to a harmonious development of its parts and functions, often in apparent opposition to or violation of the environmental conditions. This finds its vaguest and most general expression in what we call 'character' or in what systematists feel, but are often unable to describe, the 'habitus.' Its deeper manifestations, however, are of the nature of instinct and intelligence. This language may be tinged with metaphysics, not to say mysticism, but those who have finally learned to find animals most interesting when not 'fixed' in some fluid recommended in a German laboratory, or converted into skins, skeletons, shells, cadavers or fossils, will comprehend at least the intention of the writer.*

The only term hitherto suggested which will adequately express the study of animals, with a view to elucidating their true character as expressed in their physical and psychical behavior towards their living and inorganic environment, is *ethology*. This term has been employed to some extent by French zoologists and, as the writer infers from Dr. Bather's article, attempts have already been made to establish its English usage. Dahl† has advocated its introduction into Germany in the place of 'Biologie' (in the German sense) a term which in that country has been very generally preferred to Haeckel's 'œkologie.' On the other hand, the retention of 'Biologie' has been ably defended by Wasmann (*loc. cit.*), and it is probable that it will remain in general favor, notwithstanding the ambiguity of the word. This danger is perhaps not so great in Germany, where every zoologist or botanist does not style himself a 'biologist' or at least give a course of lectures in 'general biology.' Be this as it may, however, the question is one to be settled by the Germans themselves, and we are at perfect liberty to use 'eth-

* The difference between the interests of the botanists and zoologists is most clearly seen in the difference of the problems suggested by 'plant societies' and by social animals.

† 1. 'Vergleichende Untersuchungen über die Lebensweise der Aasfresser,' *Sitz. Ber. Akad. Wiss. Berlin*, II., III., 16. Jan., 1896; 2. Experimentelle statistische Ethologie,' *Verhand. Deutsch. Zool. Gesel.*, 1898, pp. 121–131; 3. 'Was ist ein Experiment, was Statistik in der Ethologie?' *Biol. Centralbl.*, 21. Bd., 1901, p. 675.

ology,' especially as the German usage of 'biology' among English or American zoologists is almost without precedent.

The word 'ethology' is singularly happy in its derivation from ἦθος, which embraces in the wealth of its connotations, all the aspects of the zoological discipline for which a concise and appropriate name is so much needed. The origin of the word ἦθος from ἔθος, custom, usage, is clearly given in Aristotle.* The general Greek usage of ἦθος, especially in the plural ἤθη, as the accustomed seat, haunt, habitat or dwelling of men or animals, admirably expresses the chorological aspect of 'ethology'; its usage in the sense of habit, manners, etc. (Lat. *consuetudo, mores*) expresses what we mean by animal behavior, while the signification of ἦθος as character, disposition, nature, etc. (Lat. *indoles, ingenium, affectus*) is well suited to express the psychological aspects of 'ethology.' Certainly no term could be more applicable to a study which must deal very largely with instincts, and intelligence as well as with the 'habits' and 'habitus' of animals. It is apparent from a moment's reflection that the term may be readily made to include all and more than is meant by 'Biologie' in the German sense, or 'œcology' in the Haeckelian sense.

There may be a possible objection to the use of 'ethology' on the ground that it has been employed in English in two senses besides the one here advocated, viz., as the name of the science of ethics and as mimicry, or pantomime.† But the latter usage appears to be quite obsolete, and an authority on moral philosophy informs the writer that he has never encountered the word 'ethology' in the sense of 'ethics.' Hence this usage must be too uncommon to prevent the zoologist from appropriating the term for technical purposes.

Father Wasmann (*loc. cit.*, pp. 398, 399) defines 'ethology' (or rather its equivalent, 'Biologie') as 'the science of the external conditions of existence which pertain to organisms as individuals and at the same time regulate their relations to other organisms and to the inorganic environment.' It therefore embraces in its restricted sense: "first, a knowledge of the mode of life of animals and plants, their nourishment, dwelling, mode of propagation, the care of offspring and their development, in so far as these present external manifestations; hence also, second, a knowledge of the life-relations that obtain between individuals of the same and different species (including all the phenomena of parasitism, symbiosis, etc.), and hence also, third, a knowledge of the conditions of existence which are essential to the life and maintenance of animals and plants." It occurs to the writer that it would be better to substitute 'general' for 'external' in this definition. Of course, 'general and special' are open to the same objections as 'external and internal' on account of the impossibility of drawing a hard and fast line between the two alternatives. But it seems better, on the whole to emphasize the former alternatives on account of the large element of general comparative psychology, physiology, morphology and embryology, which must enter into ethological investigation. 'Generality' also expresses in a more satisfactory manner the central position of 'ethology' among the remaining zoological disciplines. Whenever we undertake the detailed or exhaustive study of an ethological problem we are led imperceptibly into the details of physiology, psychology, morphology, embryology, taxonomy or

* "Τὸ γαρ ἦθος ἀπὸ τοῦ ἔθους ἐχει τὴν ἐπωνυμίαν. ἠθικὴ γὰρ καλεῖται διὰ τον ἐθίζεσθαι." 'Ethica Magna,' II., 6.2; 'Ethica Endemia,' II., 2.1. Ed. Bekker.

† Century Dictionary.

chorology, according to the particular aspect of the subject under consideration. On the other hand, the interests of all these various sciences are slowly but surely converging to a point which is not far from the center of gravity of 'ethology.' This is apparent in the 'types' and 'habitus' of the systematist and morphologist, in the conceptions of the 'individual,' in experimental embryology and the study of growth and regeneration, in the conceptions of 'adaptivity' among the 'neovitalists,' in the mystic zoology of a Maeterlinck, in the theories of 'determinate variations' and 'orthogenesis,' in recent experimental work on the origin of mutations, etc. In all this work there is apparent a turning away from the 'mechanical' and 'environmental,' a realization of the prematureness and inadequacy of all biological 'explanations' couched in terms of *existent* chemistry and physics, and an appreciation of greater depth and mystery in the life activities than had been previously conceded.

So numerous are the signs of the time that it requires little prophetic insight to discern that we are on the eve of a renascence in zoology. There have been voices crying in the wilderness for many years, and it may be well to hark back to some of these and catch the full force of their intention. First there was Goethe, who glowed with the magnificence of the problem:

"Was ist doch ein Lebendiges für ein köstliches, herrliches Ding! wie abgemessen zu seinem Zustande, wie wahr, wie seiend!"

Then there was the father of developmental science, Karl Ernst von Baer, who began to doubt whether the field he had himself cultivated with such success would yield more than a small portion of the desired harvest:

"Wissen möchten wir ob das 20. Jahrhundert nicht, wenn man die Kunst *das Leben im Leben* zu beobachten, wieder gelernt hat, über die Selbstzufriedenheit des 19. lächeln wird, mit der es glaubt, aus dem Leichnam das Leben in seiner ganzen Fülle erkennen zu können, fast vergessend, dass mit dem bildenden Leben ein handelndes innig verbunden ist, das dem Messer und dem Mikroskop sich entzieht."

And among the latest there is Jules Fabre, indefatigable observer and incomparable writer, who points to the old, sure method of all science as *the* method of 'ethology':

"Large part faite à l'anatomie, précieuse auxiliaire, que savons-nous de la bête? A peu près rein. Au lieu de gonfler avec ce rien d'abracadabrantes vessies, glanons des faits bien observés, si humbles soient."

William Morton Wheeler.

Austin, Texas,
May 17, 1902.

THE LAW OF VON BAER.

BASED ON SCHOLION V.

The writings of von Baer have been subject to much interpretation, and have yielded under the nursing hand of 'productive' scholarship, meanings which in reality they do not contain. It seems therefore worth while to reconsider what is the great generalization at which he arrived; and to those interested in the historical side of embryology, this attempt to follow the reasoning of a masterly investigator may be not unwelcome.

I.

THE PREVAILING VIEW THAT THE EMBRYO PASSES THROUGH THE ADULT STAGES OF LOWER ANIMALS.

At the time when the first volume of the 'Observations and Reflections on the Development of Animals' was published (1828), no propositions in embryology en-

joyed wider acceptance than these: That higher animals in their development from the first beginnings correspond, stage for stage, with the adult condition of lower; that the development of the individual takes place according to the same laws as that of the series; that the more highly organized ones pass in general through the adult stages of those less highly organized, so that the differences between the stages in individual development, may be referred back to the differences between persisting adult forms.

These opinions, born of the time when, excepting Malpighi and Wolff, no one had studied connectedly the earlier periods in the history of the development of any animal, could not fail to excite interest; particularly since by their aid certain malformations could readily be explained as cases arrested in development. The rampant speculations of the Lamarckians derived support from them, but the teachings of this school were as repugnant to von Baer as to many other thoughtful students.

Suppose, he says, that a fish stranded on a sandy beach were seized with the desire to walk, then, according to this school, the fins, unsuited for the perambulatory movements, would promptly shrink in breadth from disuse and in turn grow in length. These modified appendages, transferred to children and grandchildren for several thousand generations, are naturally in the end transformed into feet. Naturally, too, the fish in the meadow gasp for air, and their struggles in the end produce lungs, the only requirement being that a few generations should be exposed to the slight inconvenience of not breathing at all.

The long neck of the heron is due to the fact that his ancestors often stretched that organ in order to catch fish. Their children came into the world with elongated necks and the same evil habit, and thus gave to their offspring necks still longer, from which it follows that if our planet only reaches a ripe old age, the heron's neck will extend beyond the bounds of certain knowledge.

II.

DOUBTS AND OBJECTIONS.

(*a*) At an early time von Baer saw that the relationships between different animals could not be looked upon as representing a steady advance, which is a necessary corollary of the propositions he has set out to criticise. Above all, suspicions were generated from the fact that until that time only the development of the higher forms was known, and this incompletely. What differences their embryonic history exhibited must, if they were to find analogies anywhere, find them among the lower animals. Indeed, resemblances between the embryonic condition of certain animals, and the adult stages of others, seemed to von Baer quite necessary and without significance, since they all fall within the realm of the animal kingdom, and the variations of which the animal body is capable are determined in each case by the interrelations of the separate organs, and in these interrelations, repetitions necessarily occur.

If birds had studied their own embryonic history, and were now engaged in unraveling the structure of adult mammals and man, would not their text-books read as follows:

Those four- and two-legged animals have similar embryos, for the bones of their skulls are separate and they have no bills, as we have after five or six days of incubation. Their extremities are all pretty much alike, about the same in length as our own; not a single true feather adorns their bodies, but only a thin down, in which respect our very nestlings surpass them. Their bones are not very brittle and contain (as ours do in youth) no air; indeed,

they have no air sacs at all and their lungs are free in the pleural sacs; they are utterly devoid of a crop, and gizzard and stomach are but indistinctly delimited from one another, a condition ephemeral with us. The nails of most of them are clumsy and broad, as with us before hatching. Of all of them only the bats, which seem to be the most highly developed, possess the ability to fly. And these mammals, who for so long a time after birth are utterly helpless, and who during their whole lives can never raise themselves off the ground, claim to be more highly organized than we.

(*b*) If it were a law of nature that the development of an individual consists in passing through the adult stages of animals less highly developed, it would follow:

1. That no embryo could pass through stages which do not characterize the adult condition of some animal. There are no animals, however, which carry their food around in a yolk sac, and yet from the development of birds and certain mammals, such animals ought, according to the law, to exist.

2. Just as the environment of an embryo is related to the presence of organs which occur in no higher forms, so it makes impossible the passage through certain lower stages. Thus since all the higher embryos are bathed in water, that distinctive characteristic of insects, the tracheæ, can never develop.

3. An embryo, according to the prevailing theory, should resemble in its various stages a lower form, not merely in one particular, but in all. If at the time when the chambers of the heart are not yet separate, and the digits have not yet become distinct, the embryo is said to be in the fish stage, where is the flattened tail and all that makes up a fish and appears so early in its development?

4. There should be no ephemeral organs in lower animals which are permanent in higher ones, but there are many such, to some of which the bird embryologist has already called attention.

5. The organs in the different classes of animals should appear in the same condition in which we find them during the embryonic life of higher ones, but this is scarcely ever so.

6. Those structures found only in higher animals should appear late in their development. This, however, is by no means true. Parts of the vertebral column and the arches of the vertebræ appear in the chick earlier than any other organs. How can the chick ever resemble an invertebrate?

III.

THE RELATIONSHIPS BETWEEN DIFFERENT ADULT ANIMALS.

(*a*) The degree of development of the animal body, and the type of organization, must be clearly distinguished. The degree of development of the animal body consists in a certain amount of heterogeneity in its component parts; in diversity of tissues and of form. The more homogeneous the mass of the body, the lower the degree of development. The fishes, for example, because they have a brain, a cord and a skeleton, and present clearly the vertebrate type, are held to be superior to all invertebrates, and the advocates of the supposed law of development wonder that the bee and most insects with metamorphosis give evidence of greater skill and a more complicated life. In the bee, however, nerves and muscles are developed to such a degree that they differ from each other much more than do the same organs in fishes. Indeed the nerves and muscles of the latter seem to be soggy with the water in which they live.

(*b*) By type of organization is meant the relations existing between the organic

elements and organs on account of their positions in space, and these spacial relations are connected with certain fundamental processes of life, viz., the position of the receptive and excretory poles. Type is thus entirely different and distinct from degrees of development. The same type may be exhibited in several different degrees of development, and conversely the same degree of development may be reached in several different types. The result of a degree of development and the type gives the distinguishing characteristics of a class.

THE DOCTRINE OF TYPES.

According to this doctrine, the animal world presents four fundamental types of organization, the peripheral or radiating type, found in infusorians, medusæ, and asteroids; the segmented or length type, found in worms; the massive type, found in molluscs, and in some radiolarians and infusorians; and finally the vertebrate type, a composite, in which all types are united. Thus the vertebrate brain is built probably after the asteroid type; the viscera are certainly molluscan, and the vertebral column, without doubt, worm-like, though according to the argument in other places, distinctively vertebrate at the same time.

These four fundamental types are capable, by suppression and expansion, of many combinations, and the amount of suppression or preponderance of the different types determines classes, genera, and species. 'If it be true,' von Baer says, 'that the larger and smaller groups of animals depend on this twofold relation, between the degrees of development and the types of organization, then the opinion that there exists an uninterrupted advance from the lower to the higher is based on misconception.'

IV.

APPLICATION OF THE ABOVE DOCTRINE TO THE HISTORY OF INDIVIDUAL DEVELOPMENT.

(*a*) It is clear that a higher or a lower degree of development is the same thing as a greater or less degree of diversity in tissues and in form. The mass out of which an embryo is molded, and the body mass of the simplest animal, are very much alike, for in both there is little distinctness of form, and slight contrast of parts. If therefore we discover in the tissues of some lower animals a greater degree of diversity than in others, and place them in series according to the differences presented, we find many coincidences between the observed facts and the requirements of the genetic law implied by this series.

(*b*) These coincidences between the facts and the theoretical requirements, however, do not show that the embryo of a higher form passes gradually through the adult stages of lower ones. It seems, in fact, as though the type of each animal were immediately impressed upon its embryo, and that this governs its whole development. The history of the chick is a commentary to this statement.

The first organs to be distinguished in the germ are those of the vertebrate type, and it is clear that after their appearance resemblance to an invertebrate can no longer be held. In the beginning of their development, all classes of vertebrates are very similar, and so we can say that the embryo of a vertebrate is from the beginning a vertebrate and has at no time any resemblance to an invertebrate. An adult animal, having the vertebrate type and such slight diversity of tissues and distinctness of form as the vertebrate embryo, is unknown, and so the embryos of vertebrates in their development do not pass through the adult stages of any known animals.

(*c*) 'Is there then no law of individual development?' asks von Baer. He believes there is and bases it on the following considerations:

The embryos of mammals, birds, lizards and snakes present such similarities in their entirety as well as in the development of corresponding parts that except for differences in size it would be difficult to distinguish them.

The further we go back in the history of development, the greater do we find those similarities, and only gradually do those special characters emerge from the general type which distinguishes the smaller groups of animals. To this the history of the chick in every stage of its development bears witness.

In the beginning, when the back closes, it is a vertebrate and nothing more. When the embryo becomes more and more separated from the yolk; when the gill clefts close and when the urinary sac grows out, it becomes a vertebrate unsuited for free life in the water. Only later, a difference in the extremities is recognizable and the bill appears; the lungs move upwards and the air sacs are established as rudiments. Now there is no longer any doubt that the form is a bird. While the avian characteristics become augmented by development of the wings and air sacs, and by the fusion of the carpel cartilages, the webs of the feet disappear and we have a terrestrial bird. Later when the crop is developed and the nasal scale appears, the terrestrial bird takes on the characters of the *Gallinæ* and finally those of the domestic fowl.

(*d*) Briefly, we may say that the point of greatest resemblance in the development of two animals is remote in proportion to the amount of difference they exhibit in their adult condition. The differences between the long-tailed and the short-tailed crabs are not very great. The crayfish has in the middle of its embryonic life a tail short in proportion to the broad sternum, and it is difficult to distinguish at this stage from the short-tailed crabs, which, according to Cavolini's figures, are in their embryonic condition comparatively long-tailed. The further we go back in the history of development, the greater do we find the similarity between the feet and the organs of mastication. We have thus not only an approach to the fundamental type, but a resemblance to the Stomatopoda, the *Amphipoda* and the *Isopoda*, which in their fully developed state differ more from the *Decapoda* than these do among themselves. To this may be added that in the *Decapoda* according to Rathke, the heart appears spindle-shaped, and many other points of similarity, so far unrecognized, must exist. Still earlier, when the feet are present as small laterally budding knobs, and the gills are not yet visible, a resemblance with true insects in their embryonic condition is not to be denied.

These considerations bring us to the question whether there is not, early in the history of development, a stage in which the embryos of vertebrates resemble those of invertebrates. In another place, von Baer shows that even the series of segmented animals begins development with a primitive streak, and that therefore during this brief period there is a resemblance between them and the early stages of vertebrates. In the germ, all embryos developed from a true egg probably resemble each other and in this lies a strong reason for considering the germ as the animal itself.

(*e*) The further back we go in development, the more points in common do we find in very different animals, and so the question arises whether in the beginning all are not fundamentally alike and whether there does not exist a common ancestor. All true eggs appear to have a

distinct sheet-like germ, which seems to be lacking to the germ grains so far as their development is known. These latter are in the beginning solid, but their first act of independent life seems to be a hollowing-out by which they become converted into thick-walled vesicles, observed in the case of *Cercaria* and *Bucephalus*. The germ of a true egg is also to be looked upon as a vesicle, which in the case of birds only gradually grows around the yolk, being supplemented in the beginning by the vitelline membrane. Since, however, the germ is the undeveloped animal itself, we cannot assert, without good reason, that the simple vesicular form is the common ancestor from which all animals are descended. The germ grain goes over into this primitive condition on account of its own inherent power; the egg, only after its female nature has been neutralized by fertilization. Not until this has occurred does the separation into germ and yolk, or body and nutrient stuff, take place.

(*f*) If in order to find resemblance between two animals, we must go back in the history of development a distance proportional to the amount of difference they display in their adult condition, we must recognize as laws of individual development:

1. That those characteristics common to a large group of animals appear earlier in their development than those which characterize the members of the group individually.

2. That from the general, the less general is formed, until what is most special appears.

3. That the embryo of every animal, instead of passing through the adult stages of others lower in the scale, in reality grows increasingly different from these.

4. That the embryo of a higher animal never does resemble the adult of a lower one, but only its embryo.

It is only because the less highly developed animals go little beyond their embryonic condition, that they present certain points of similarity with the embryos of higher forms. These resemblances therefore do not indicate the existence of a limiting condition determining the course of the development of the higher forms, but find their explanation in the organization of the lower ones.

(*g*) These facts are illustrated graphically in a table showing the advance from the lowest grade of development to the highest. From this schema it is clear that an embryo cannot be maintained to pass in its development through the whole series of animals, because it cannot pass from one fundamental type over into another. Then again an embryo in its development does not pass through another form but only through the region of indifference between that form and its own adult condition. Thus the further the development proceeds, the narrower does the region of indifference become. The *schema* also demonstrates that an embryo in the beginning is an indifferent vertebrate, then an indifferent bird, and so on. Since in its progression from one region of indifference to the next it is becoming internally more and more perfect, it is at the same time also becoming a more and more highly developed animal.

The view here advocated differs from the one generally held, in that this is based on an unproved assumption and derives support from the neglect of the important distinction between type of organization and degree of development.

The embryo is gradually formed by progressive diversification of tissues and of form, and for this reason the younger it is, the more nearly does it resemble slightly developed animals. Different animals vary more or less from the basal type which is nowhere pure but occurs

only in definite modifications. Fishes are nearer the type than mammals and especially man. Naturally therefore the embryos of mammals resemble fishes. If we recognize in the fish merely the slightly developed vertebrate (which is the unfounded assumption) we must interpret the mammal as a highly developed fish, and then of course it is consistent to say that the embryo of a vertebrate is at first a fish. For this reason the prevailing view of the law of individual development necessarily implies a progressive series in the animal kingdom. But the fish is more than an imperfect vertebrate. It has undoubted piscan characters as its development abundantly shows, and this development, as in all animals, is governed by two conditions:

1. By progressive diversification of tissues and of form, accompanied,

2. By the passage from a general, indifferent and indefinite state into a definite and particular one.

Otto C. Glaser.

Johns Hopkins University.

MEMBERSHIP OF THE AMERICAN ASSOCIATION.

The following have completed their membership in the American Association for the Advancement of Science during the month of May:

Herman R. Ainsworth, M.D., Addison, N. Y.

Chas. E. Allison, M.D., Elysburg, Pa.

Howard S. Anders, M.D., 1836 Wallace St., Philadelphia, Pa.

Winslow Anderson, M.D., 1025 Sutter St., San Francisco, Cal.

Wm. J. Asdale, M.D., 5523 Ellsworth Ave., Pittsburg, Pa.

Adolph Barkan, M.D., 14 Grant Avenue, San Francisco, Cal.

Guido Bell, M.D., 431 E. Ohio St., Indianapolis, Ind.

J. Mortimer Bessey, M.D., 1814 Adams St., Toledo, Ohio.

Julius C. Bierwirth, M.D., 137 Montague St., Brooklyn, N. Y.

Joseph W. Blankinship, Ph.D., State College, Bozeman, Montana.

Anthony J. Boucek, M.D., 624 Chestnut St., Allegheny, Pa.

Norman Bridge, M.D., 100 Grand Ave., Pasadena, Cal.

Wallace A. Briggs, M.D., 1300 I St., Sacramento, Cal.

George M. Brill, 1134 Marquette Bldg., Chicago, Ill.

Philip K. Brown, M.D., 1303 Van Ness Ave., San Francisco, Cal.

Charles C. Browning, M.D., Highland, Cal.

James I. Buchanan, 6108 Walnut St., Pittsburg, Pa.

W. J. Burdell, M.D., Lugoff, S. C.

Leroy S. Chadwick, M.D., 1824 Euclid Ave., Cleveland, Ohio.

Stanford E. Chaillé, M.D., P. O. Drawer 261, New Orleans, La.

William Cleburne, 1219 South 6th St., Omaha, Nebr.

Thomas U. Coe, M.D., Bangor, Maine.

Wm. H. Coster, Shady Ave., above 5th Ave., Pittsburg, Pa.

H. Holbrook Curtis, M.D., Madison Ave., New York, N. Y.

J. Y. Dale, M.D., P. O. Box 14, Lemont, Pa.

Nathan S. Davis, M.D., 65 Randolph St., Chicago, Ill.

Gordon K. Dickinson, M.D., 278 Montgomery St., Jersey City, N. J.

Wm. S. Disbrow, M.D., 151 Orchard St., Newark, N. J.

George Dock, M.D., 1014 Cornwell Place, Ann Arbor, Mich.

Charles W. Dulles, M.D., 4101 Walnut St., Philadelphia, Pa.

Henry B. Dunham, M.D., State Sanatorium, Rutland, Mass.

Lehman H. Dunning, M.D., 224 N. Meridian St., Indianapolis, Ind.

Lewis L. Dyche, 1611 Mass. St., Lawrence, Kansas.

James B. Eagleson, M.D., 512 Burke Bldg., Seattle, Wash.

Adalbert Fenyes, M.D., P. O. Box W, Pasadena, Cal.

George E. Fisher, University of Pennsylvania, Philadelphia, Pa.

Robert Fletcher, Thayer School of Civil Engineering, Hanover, N. H.

Mary Gage-Day, M.D., 207 Wall St., Kingston-on-Hudson, N. Y.

Ramon D. Garcin, M.D., 2618 E. Broad St., Richmond, Va.

Geo. W. Cargill, Attorney-at-Law, Charleston, W. Va.

Joseph E. Garland, M.D., 17 Pleasant St., Gloucester, Mass.

Harold Gifford, M.D., 405 Kasbach Block, Omaha, Neb.

Charles C. Godfry, M.D., 753 Lafayette St., Bridgeport, Conn.

Douglas Graham, M.D., 74 Boylston St., Boston, Mass.

Chas. L. Greene, M.D., 150 Lowry Arcade, St. Paul, Minn.

Ernest C. Grosskopf, M.D., Wauwatosa, Wis.

J. Underwood Hall, M.D., 216 Autumn St., San Jose, Cal.

H. Tuthill Hallack, M.D., Alcott, Colo.

James C. Hancock, M.D., 43 Cambridge Place, Brooklyn, N. Y.

Charles P. Hartley, Dept. of Agriculture, Washington, D. C.

Chas. L. Heisler, 909 West 8th St., Erie, Pa.

Jane Lord Hersom, M.D., 106 Pine St., Portland, Maine.

Gershom H. Hill, M.D., Independence, Iowa.

C. M. Hobby, M.D., Iowa City, Iowa.

Charles Holt, 255 West 45th St., New York, N. Y.

H. Hopeman, M.D., Minden, Nebr.

Wm. Lee Howard, M.D., 1126 N. Calvert St., Baltimore, Md.

Chas. H. Hunter, M.D., 13 Syndicate Block, Minneapolis, Minn.

Ellsworth Huntington, Highland St., Milton, Mass.

John H. Jackson, M.D., 155 Franklin St., Fall River, Mass.

Victor H. Jackson, M.D., 240 Lenox Ave., New York, N. Y.

William Jefson, M.D., Sioux City, Iowa.

George F. Jewett, M.D., Forman, N. Dak.

George B. Johnston, M.D., 407 E. Grace St., Richmond, Va.

George F. Keene, M.D., State Hospital for Insane, Howard, R. I.

Francis D. Kendall, M.D., 1309 Plain St., Columbia, S. C.

F. P. Keppel, Columbia University, New York City.

Julius E. Kinney, M.D., 1427 Stout St., Denver, Colo.

Samuel Kirkpatrick, M.D., Selma, Ala.

Wm. H. Knight, 2 Bryson Block, Los Angeles, Cal.

Frederick Kolbenheyer, M.D., 2006 Lafayette Ave., St. Louis, Mo.

Burton S. Lanphear, Iowa State Coll., Ames, Iowa.

Charles L. Leonard, M.D., 1930 Chestnut St., Philadelphia, Pa.

John E. Luckey, M.D., Vinton, Iowa.

James H. McBride, M.D., Pasadena, Cal.

M. Virginia McCune, M.D., 506 West John St., Martinsburg, W. Va.

Charles F. McGahan, M.D., Aiken, S. C.

Archibald MacLaren, M.D., 350 St. Peter St., St. Paul, Minn.

Egbert W. Magruder, Dept. of Agriculture, Richmond, Va.

Stephen A. Mahoney, M.D., 206 Maple St., Holyoke, Mass.

Thomas H. Manley, M.D., 115 West 49th St., New York, N. Y.

James P. Marsh, M.D., 1828 Fifth Ave., Troy, N. Y.

Clara Marshall, M.D., 1712 Locust St., Philadelphia, Pa.

Lewis D. Mason, M.D., 171 Joralemon St., Brooklyn, N. Y.

John F. Meyer, Ardmore, Pa.

Wm. F. Mitchell, M.D., Lancaster, Mo.

Max W. Morse, Ohio State Univ., Columbus, Ohio.

Murray G. Motter, M.D., 30th and U Sts., Washington, D. C.

Harold N. Moyer, M.D., 103 State St., Chicago, Ill.

Walter H. Neilson, M.D., Milwaukee, Wis.

Spencer Otis, 1502 Fisher Bldg., Chicago, Ill.

Frederick A. Packard, M.D., 258 S. 18th St., Philadelphia, Pa.

Arnold Peskind, M.D., 1354 Willson Ave., Cleveland, Ohio.

Marcel Pietrzycki, M.D., Starbuch, Wash.

Miles F. Porter, M.D., 207 W. Wayne St., Ft. Wayne, Ind.

Richard B. Potter, M.D., West Palm Beach, Fla.

Samuel C. Prescott, Mass. Inst. Tech., Boston, Mass.

John B. Probasco, M.D., 175 East Front St., Plainfield, N. J.

Charles Puryear, College Station, Texas.

Burton A. Randall, M.D., 1717 Locust St., Philadelphia, Pa.

Joseph Rauschoff, M.D., Cincinnati, Ohio.

Chas. H. Reckefus, Jr., M.D., 506 N. 6th St., Philadelphia, Pa.

Mark W. Richardson, M.D., 90 Equitable Building, Boston, Mass.

David Riesman, M.D., 326 South 16th St., Philadelphia, Pa.

Arthur Curtis Rogers, M.D., Fairbault, Minn.

John T. Rogers, M.D., Lowry Arcade, St. Paul, Minn.

Milton J. Rosenau, M.D., Marine-Hospital Service, Washington, D. C.

Wm. H. Ruddick, M.D., South Boston, Mass.

Frederick D. Ruland, M.D., Westport, Conn.

Frank Schlesinger, Ukiah, Cal.

Carl Schwalbe, M.D., 1002 South Olive St., Los Angeles, Cal.

Henry L. Shaw, M.D., 19 Commonwealth Ave., Boston, Mass.

John C. Simpson, M.D., Govt. Hospital for Insane, Washington, D. C.

Wm. T. Smith, M.D., Dartmouth Medical Coll., Hanover, N. H.

Haldor Sneve, M.D., Lowry Arcade, St. Paul, Minn.

Albert E. Sterne, M.D., 'Norways,' Indianapolis, Ind.

J. Clark Stewart, M.D., 1628 Fifth Ave. South, Minneapolis, Minn.

Charles G. Stockton, M.D., 436 Franklin St., Buffalo, N. Y.

J. Edward Stubbert, M.D., 25 E. 45th St., New York, N. Y.

H. Longstreet Taylor, M.D., 75 Lowry Arcade, St. Paul, Minn.

Lewis H. Taylor, M.D., 83 S. Franklin St., Wilkesbarre, Pa.

Hugh L. Thompson, Waterbury, Conn.

Wm. J. Todd, M.D., Mt. Washington, Baltimore, Md.

Fenton B. Turck, M.D., 362 Dearborn Ave., Chicago, Ill.

James Tyson, M.D., 1506 Spruce St., Philadelphia, Pa.

M. C. Van Gundy, Herron Hill Laboratory, Center Ave. and Craig St., Pittsburg, Pa.

Hiram N. Vineberg, M.D., 751 Madison Ave., New York City.

John H. Voje, M.D., Oconomowoc, Wis.

John W. Wade, M.D., 318 N. Second St., Millville, N. J.

Edwin O. Weaver, Wittenberg Col., Springfield, Ohio.

Frank G. Wheatley, M.D., 47 Adams St., North Abington, Mass.

Frederick M. Wilson, M.D., 834 Myrtle Ave., Bridgeport, Conn.

Gustave Windesheim, M.D., 255 Main St., Kenosha, Wis.

Max E. Witte, M.D., Clarinda, Iowa.

Thomas D. Wood, M.D., Columbia University, New York, N. Y.

SCIENTIFIC BOOKS.

La géométrie Non-Euclidienne. Par P. Barbarin. Paris, C. Naud. Scientia, Février. 1902. Phys. Mathématique, No. 15. Pp. 79.

It is peculiarly appropriate that from Bordeaux, made sacred for non-Euclidean geometry by Hoüel, should emanate this beautiful little treatise, decorated with a 'gravure' reproducing part of a manuscript of Euclid, also with the official portrait of Lobachevski, but best of all, with a portrait of Riemann.

It begins from the hackneyed position: 'Experience therefore it is which has furnished to the ancient geometers a certain number of primitive notions, of axioms, or fundamental postulates put by them at the basis of the science.' But now we know there never was any pure receptivity. In all thinking enters a creative element. Every bit of experience is in part created by the subject said to receive it, but really in great part making it.

Professor Barbarin continues: 'From the epoch of Euclid, this number has been reduced to the strict minimum necessary, and all the others not comprised in this list, being capable of demonstration, are put in the class of theorems.' Now we know that Euclid omits to notice many of the assumptions he unconsciously employs, for example all the 'betweenness assumptions,' while Hilbert has at last rigorously demonstrated Euclid's assumption 'All right angles are equal,' and in turn one of Hilbert's assumptions has just been proved (see *Amer. Math. Monthly,* April, 1902, pp. 98–101).

The 'Elements' of Euclid, says Professor Barbarin, enjoyed throughout all the middle ages and still enjoy a celebrity that no other work of science has attained; this celebrity

is due to their logical perfection, to the admirable concatenation of the propositions, and to the rigor of the demonstrations. 'Il mit dans son livre,' says Montucla, 'cet enchaînement si admiré par les amateurs de la rigueur géométrique.' "In vain," he adds, "divers geometers whom this arrangement has displeased, have attempted to better it.

"Their vain efforts have made clear how difficult it is to substitute for the chain made by the Greek geometer another as firm and as solid."

'This opinion of the historian of mathematics,' says our author, 'retains all its value even after the researches which geometers have undertaken for about a century to submit the fundamenal principles of the science to an acute and profound examination.' I add that the remarkable discoveries of Dehn (see SCIENCE, N. S., Vol. XIV., pp. 711–712) prove an unexpected superiority for Euclid over all successors down to our very day, and suggest the latest advance, which, though as yet unpublished, exists, for under date of April 2, 1902, Hilbert writes me: 'In einer andern Arbeit will ich die Lobatschefski'sche Geometrie in der ebene unabhangig von Archimedes begrunden.' That is, Hilbert will found Bolyai's geometry as he has Euclid's, without any continuity assumption.

To get the benefit of this brilliant achievement, I am holding back my own book on this fascinating subject.

Says Hilbert in his unpublished Vorlesung ueber Euklidische Geometrie, "The order of propositions is important. Mine differs strongly from that usual in text-books of elementary geometry; on the other hand, it greatly agrees with Euclid's order.

"So fuehren uns diese ganz modernen Untersuchungen dazu, den Scharfsinn dieses alten Mathematikers recht zu wuerdigen und aufs hoechst zu bewundern."

Again, *à propos* of Euclid's renowned parallel postulate, Hilbert says: "What sagacity, what penetration the setting up of this axiom required we best recognize if we look at the history of the axiom of parallels. As to Euclid hmself (circa 300 B. C.) he, *e. g.*, proves the theorem of the exterior angle before introducing the parallel axiom, a sign how deeply he had penetrated *in den Zusammenhang der geometrischen Saetze.*"

Professor Barbarin repeats the exploded error of attributing to Gauss the discovery of the non-Euclidean geometry in 1792. In the introduction to my translation of Bolyai's 'Science Absolute of Space,' pp. viii–ix, is a letter from Gauss, on which I there remark: "From this letter we clearly see that in 1799 Gauss was still trying to prove that Euclid's is the only non-contradictory system of geometry, and that it is the system regnant in the external space of our physical experience. The first is false; the second can never be proven."

In 1804 Gauss writes that in vain he still seeks the unloosing of this Gordian knot.

Again, with the date April 27, 1813, we read: "In the theory of parallels we are even now not farther than Euclid was. This is the 'partie honteuse' (shameful part) of mathematics, which soon or late must receive a wholly different form." Thus in 1813 there is in Göttingen still no light.

But in 1812 in Charkow, the non-Euclidean geometry already had been for the first time consciously created by Schweikart, whose summary characterization of it is given in SCIENCE, N. S., Vol. XII., pp. 842–846. This he communicated to Bessel and sent to Gerling and afterward to Gauss in 1818, so that it may claim to be the first *published* (not printed) treatise on non-Euclidean geometry.

By this time Gauss had progressed far enough to be willing to signify *privately* his acceptance of Schweikart's doctrines.

On p. 15, Barbarin makes a brief argument for Euclid's axiom, 'All right angles are equal.'

This argument was good before Hilbert and Veronese, since this axiom can never be proved by superposition. It is already a consequence of the assumptions preliminary to motion. This profounder analysis Barbarin has not attained to. He still uses as a postulate and supposes indispensable 'l'indeformabilité des figures en déplacement.' What Jules Andrade calls 'cette malheureuse et illogique définition' of Legendre, 'the shortest path between two points is a straight

line,' Barbarin puts as an elementary proposition!

Manning also, p. 2, assumes it, thus invalidating and making ephemeral his pretty little 'Non-Euclidean Geometry' (Ginn & Co., 1901). Barbarin then proceeds to classify geometries by Saccheri's three hypotheses, the hypothesis of obtuse angle, the hypothesis of right angle, the hypothesis of acute angle, or that the angle sum of a rectilinear triangle is greater than, equal to, less than two right angles.

But the remarkable discoveries of Dehn have now shown that this classification is invalid.

Barbarin says, p. 16, 'Saccheri proves that the hypothesis of the obtuse angle is incompatible with postulate 6' of Euclid.

Dehn dissipates this supposed incompatibility by actually exhibiting a new geometry in which they amicably blend, which he calls the non-Legendrean geometry.

In the same way, the hypothesis of right angle amalgamates with the contradiction of Euclid's parallel-postulate in a geometry which Dehn calls semi-Euclidean. As Dehn states this result: There are non-Archimedian geometries in which the parallel-axiom is not valid and yet the angle-sum in every triangle is equal to two right angles. Thus the theorem (Legendre, 12th Ed., I., 23; Barbarin, p. 25): 'If the sum of the angles of every triangle is equal to two right angles the fifth postulate is true,' is seen to break down.

Manning's 'Non-Euclidean Geometry,' though it says (p. 93), 'The elliptic geometry was left to be discovered by Riemann,' gives only the single elliptic.

It never even mentions the double elliptic, or spherical or Riemannian geometry, which Killing maintains was the only form which ever came before Riemann's mind. If so, then Barbarin's book is like Riemann's mind. The Riemannian, as distinguished from the single elliptic, is the only form which appears in it. Killing was the first who (1879, *Crelle's Journal,* Bd. 83) made clear the difference between the Riemannian and the single elliptic space (or as he calls it, the polar form of the Riemannian).

Klein championed the single elliptic. Manning knows no other.

Professor Simon Newcomb, like Manning, deals only with the single elliptic in his treatise: 'Elementary theorems, relating to the geometry of a space of three dimensions and of uniform positive curvature *in the fourth dimension.*'

The last four words F. S. Woods replaces by seven dots in his article 'Space of constant curvature' (*Annals of Math.,* Vol. 3, p. 72), though blaming Professor E. S. Crawley for the error they contain.

Newcomb's also was the unfortunate conceit which dubbed this 'A Fairy-tale of Geometry,' a point of view from which he is still suffering in his latest little unburdening in *Harper's Magazine.*

Just so Lobachevski had the misfortune to call his creation 'Imaginary Geometry.'

Contrast John Bolyai's 'The Science Absolute of Space.'

In single elliptic space every complete straight line is of finite constant length πk.

Every pair of straight lines intersect and return again to their point of intersection, but have no other point in common.

In the so-called spherical space, that is the Riemannian space, two straight lines always meet in two points (opposites, or antipodal points) which are πk from each other.

The single elliptic makes the plane a unilateral or double surface, so that two antipodal points would correspond to one point, but to opposite sides of this one-sided plane with reference to surrounding three-dimensional elliptic space.

The geometry for two-dimensional Riemannian space coincides completely with pure spherics, that is with spherics established from postulates which make no reference to anything off of the sphere, inside or outside the sphere. Hence the great desirability of a treatise on pure spherics. It would at the same time be true and available for Euclidean and for Riemannian geometry.

Yet its relations to three-dimensional Euclidean and three-dimensional Riemannian space would differ radically.

Through every Riemannian straight line

passes an infinity of planes also Riemannian, and in each of these this straight has a determined and distinct center; but the straight is independent of the planes, and is defined by the postulates.

Now in the sphere the great circle and the one *pseudo*-plane which contains and fixes it, namely the sphere, are inseparable, since any portion, however minute, of either determines all the other as well as its center and radius.

In the single elliptic geometry the elliptic straight line does not divide the elliptic plane into two separated regions. We can pass from any one point of the plane to any other point without crossing a given straight in it. Starting from the point or intersection of two straights and passing along one of them a certain finite length, we come to the intersection point again without having crossed the other straight. Hence we can pass from what seems one side of the straight line to what seems the other without crossing it, that is, it is uni-lateral or double.

This single elliptic geometry is never mentioned in Barbarin's book; just as the Riemannian is never mentioned in Manning's book. First take your choice, then buy your non-Euclidean geometry.

On p. 36, Barbarin gives to Gauss the honor which belongs to Wallis of being the first to remark that the existence of unequal similar figures is equivalent, in continuous space, to the parallel postulate.

In Chapter VII., 'Les Contradicteurs de la géométrie non-euclidienne,' Professor Barbarin makes with unanswerable vigor the argument which I gave in my 'Report on Progress in Non-Euclidean Geometry' (SCIENCE, N. S., Vol. X., pp. 545–557).

There I quoted Whitehead who was the first to publish (March 10, 1898) "the extension of Bolyai's theorem by investigating the properties of the general class of surfaces in any non-Euclidean space, elliptic or hyperbolic, which are such that their geodesic geometry is that of straight lines in a Euclidean plane.

"Such surfaces are proved to be real in elliptic as well as in hyperbolic space, and their general equations are found for the case when they are surfaces of revolution.

"In hyperbolic space, Bolyai's limit-surfaces are shown to be a particular case of such surfaces of revolution.

"The same principles would enable the problem to be solved of the discovery in any kind of space of surfaces with their 'geodesic' geometry identical with that of planes in any other kind of space."

Now not only the strikingly important problem solved by Whitehead, but also the analogous problem indicated had both been solved by Barbarin and presented three months before to the Académie Royale de Belgique; but these investigations were only published after the appearance of my Report (October 20, 1899). They, as Barbarin says, p. 63, 'bring out in a striking manner the absolute independence of the three systems of geometry, which are able each to get everything from its own resources without need of borrowing anything from the others.' In each of the three spaces, Euclidean, Bolyaian, Riemannian, there exist surfaces whose geodesics have the metric properties of the straights of the two other spaces.

But the book in which these beautiful researches are published: 'Etudes de géométrie analytique non euclidienne par P. Barbarin, Bruxelles,' 1900, Hayez, pp. 168, has other titles to universal recognition.

Notwithstanding the ever-present example of Euclid, who never uses a construction or a figure which he has not shown to follow deductively from his two postulated figures, the straight and the circle, an insidious error crept into geometry, taught by Beman and Smith, who should know better, in the following words: (See their 'Geometry,' 1899, p. 70, § 112) "*Note on Assumed Constructions.*— It has been assumed that all constructions were made as required for the theorems.

"Thus an equilateral triangle has been frequently mentioned, although the method of constructing one has not yet been indicated, a regular heptagon has been mentioned, and reference might be made to certain results following from the trisection of an angle, although the solutions of the problems, to construct a regular heptagon, and to trisect any angle, are impossible by elementary geom-

etry. But the possibility of solving such problems has nothing to do with the logical sequence of the theorems." This is a fundamental blunder.

The construction so glibly assumed, to pass a circle through any three non-co-straight points, is equivalent to the assumption of the world-renowned parallel postulate, and thus has everything in the world to do with the sequence of the theorems. The assumed construction of a triangular from three sects which are to be its sides, by the method of Beman and Smith, p. 76, is equivalent to the assumption of the Archimedes postulate, which again has everything to do with the logical sequence of the theorems. In fact just this assumption makes ephemeral the beautiful method of Saccheri used in the book we are reviewing.

Hence we can appreciate that astounding achievement of Bolyai's young genius, his § 34, where he solves for his universe, Eu., I., 31. To draw a straight line through a given point parallel to a given straight line. His brilliant lead was followed more than half a century later by Gerard, but it is Barbarin who has ended the matter by deducing from certain very simple constructions of the trirectangular quadrilateral all the fundamental plane constructions.

In Chapter VIII. (La géométrie physique,' § 30 'La forme géométrique de notre univers') our author stresses the idea, that even if our universe were exactly Euclidean, it would be forever impossible for us to demonstrate this. As I said in my 'Non-Euclidean Geometry for Teachers,' p. 14, "If in the mechanics of the world independent of man we were absolutely certain that all therein is Euclidean and only Euclidean, then Darwinism would be disproved by the reductio ad absurdum. All our measurements are finite and approximate only. The mechanics of actual bodies in what Cayley called the external space of our experience, might conceivably be shown by merely approximate measurements to be non-Euclidean, just as a body might be shown to weigh more than two grams or less than two grams, though it never could be shown to weigh precisely, absolutely two grams."

Our author suggests the following experiment for proving our space non-Euclidean: From a point trace six rays sixty degrees apart. On them successively mark off the sects OA_0, OA_1, OA_2, $\cdots$, OA_n, of which each is the projection of the following. If we finish by finding between OA_n and 2^nOA_0 a difference of constant sense and greater than imputable to error of procedure, our universe is non-Euclidean.

In conclusion this beautiful little book has the advantage of being the production of an active and fertile original worker in the domain of which it treats. His 'Géométrie général des espaces' (1898), his 'Sur le paramètre de l'univers' and 'Sur la géométrie des êtres plans' (1901), 'Le cinquième livre de la métagéometrie,' (1901), 'Les cosegments et les volumes en géométrie non euclidienne' (1902), and his 'Poligones réguliers spheriques et non-euclidiens,' shortly to appear in that virile young monthly *Le Matematiche*, and which I had the advantage of reading in manuscript, show that Bordeaux is honored by a worthy successor of Hoüel, so universally beloved.

George Bruce Halsted.
Austin, Texas.

Lamarck, The Founder of Evolution, His Life and Work, with Translations of His Writings on Organic Evolution. By Alpheus S. Packard, M.D., LL.D. New York, London and Bombay, Longmans, Green & Co. 1901. Pp. xii+451.

This appears to the reviewer to be a noteworthy book; he has read it from cover to cover with so much pleasure that he ventures to predict that it will prove a source of satisfaction to that large body of readers who are interested in the rise of evolutionary thought.

Larmarck lived in advance of his age and died comparatively unappreciated.

Although quiet and uneventful, his life was a busy one, and, as sketched by Dr. Packard, his noble character, his generous disposition and his deep intellectuality are well brought out.

His devoted and loyal daughter, Cornelie, without whose assistance his later works could

not have been prepared, encouraged her father in the days of his blindness and neglect, by saying 'La postérité vous honorera.' And this has come true. Lamarck, who struggled with poverty and other depressing conditions, whose views were laughed to scorn by Cuvier, and neglected by the intellectual leaders of his time, is now receiving honor and recognition. His original and philosophical mind dealt with some of the burning questions of our day, and he is now placed above Cuvier as a thinker, and heralded, by many, as the most colossal figure in the history of the philosophy of organic nature, between Aristotle and Darwin. This fresh interest in Lamarck's views makes Dr. Packard's book especially timely.

A number of new biographical facts are added to the few that have been generally known, and the book is illustrated with four portraits of Lamarck, pictures of his birthplace, the house in which he lived in Paris, etc. In reference to the analysis of his writings the author says: 'As regards the account of Lamarck's speculative and theoretical views, I have, so far as possible, preferred, by abstracts and translations, to let him tell his own story, rather than to comment at much length myself on points about which the ablest thinkers and students differ so much.' This part of the author's task has been especially well done. Nowhere else can one find in a single volume such a comprehensive survey of Lamarck's theoretical writings.

The growth and essential features of his theory of organic evolution are shown by ample quotations. This theory was unfolded in 1800 and fully expounded in 1809 in the well-known 'Philosophie Zoologique.' The various expressions of his views in 1800, 1802, 1803 and 1806, as leading up to the latter work, are well illustrated, and seventy-six pages are devoted to quotations from the 'Philosophie Zoologique.'

Several current misconceptions are corrected, as for example—the earliest expression of Lamarck's views, as far as his published writings show, was in 1800, in the introductory lecture to his course on the invertebrates, not, as commonly believed, in his 'Recherches sur l'Organisation des Corps Vivans,' published in 1802. Incidentally, also, in reference to Buffon, it is shown that his opinions on the variability of species were not separated into three periods, but that from the time he began to express his views on that matter, to the end of his life, he was an advocate of the mutability of species.

Lamarck's work is treated from all sides; in addition to the exposition of his views on organic evolution, there are chapters on his work in botany, geology, invertebrate paleontology, general physiology and biology, zoology, his thoughts on morals and on the relation of science and religion, and on the relation of his evolutionary views to those of Buffon, St. Hilaire and Erasmus Darwin. There is also a fine chapter on Neolamarckism.

Thoroughness and breadth are notable features in this account of Lamarck and his life work.

WILLIAM A. LOCY.

SOCIETIES AND ACADEMIES.

EIGHTH REGULAR MEETING OF THE BOTANICAL SOCIETY OF WASHINGTON.

THE eighth regular meeting of the Botanical Society of Washington was held at the Portner Hotel, May 24, 1902, with President A. F. Woods in the chair. At the conclusion of the business meeting, Dr. B. M. Duggar, chairman of program for the evening, was called upon to preside.

Mr. E. L. Morris called attention to specimens of *Trillium* found near Great Falls of the Potomac River which produced long-petioled simple leaves from the rootstock. While recent manuals state that this is occasionally true for the genus, the speaker had failed to find specimens in any herbaria examined which exhibited this character.

Mr. M. B. Waite stated that the ordinary two weeks' interval had proved too long in spraying apple trees for bitter rot. In experiments the present season in Virginia, the third treatment was made just after the petals had fallen and while the trees were moderately covered with foliage. Two weeks from this time the trees were found to have made

a very rapid growth of six to ten inches, and three or four new full-grown leaves had developed on each twig, which were, of course, unprotected by the spray. In a few cases these leaves had become infected with fungi, probably the bitter rot fungus. At the time of the fourth treatment, these leaves were, of course, thoroughly covered with the mixture and protected from further infection, but it is interesting to know that the two weeks' interval at this period of rapid leaf formation was long enough for leaves to form and become infected with the fungus before they could be protected by the spray. The inference is that the interval between the third and the fourth spraying should in this case be shortened.

Mr. Wm. A. Taylor called attention to some field experiments recently made, to ascertain in a practical way to what extent bees are responsible for the spread of pear blight. In these experiments, which were conducted by Mr. Charles Downing in Kings County, Cal., where blossom blight was very destructive last year, the members of the association of bee keepers agreed to remove their bees to a minimum distance of two miles from the pear orchards for the blooming season. It was found during the blooming season that a considerable number of swarms were left in the area in question, including one lot of thirty or forty swarms that had been overlooked. Certain trees of P. Barry, Clairgeau, and Bartlett pears were covered with mosquito netting before the blossoms opened, to exclude all the larger insects, including bees. When the trees blossomed it was found that the trees of P. Barry and Clairgeau, which blossomed early, when nearby orchards of apricots and peaches were in bloom, were little visited by bees. Both covered and uncovered trees of these varieties, to the number of 3,000, set a full crop of fruit, with no blight infection except on a few late blossoms. The uncovered Bartletts, which blossomed later, beginning just before the peaches and apricots were through blooming, were well covered with bees almost from the start. The blossoms on the uncovered Bartlett trees were badly blighted, and very little fruit set on them except from the first blossoms, which opened before the bees began their visits. On the covered Bartlett trees more fruit set than on any other Barlett trees in the orchard. Some blossom blight appeared on the covered trees, but upon examination some dead bees were found inside the netting, which had been slightly torn by storms.

Mr. Downing estimates the financial loss on his Bartlett pears last season due to blossom blight at $10,000, and his loss on the same variety this year from the same cause at 1,000 tons of fruit on 9,000 trees. He concludes that so long as there are blight-infected pear trees in his locality the crop of Bartletts will be destroyed if the bees have access to them. The fact that the covered trees set fair crops of fruit appears to indicate that cross-pollination of Bartletts was not necessary in that locality this season.

In the discussion of pear blight in California, Mr. Waite stated that the blight bacteria were usually carried from the gummy exudation of hold-over cases by flies and wasps, flies being the principal agents in the transportation of the virus. After the first blossoms have been infected in this manner, the regular flower-visiting insects, of which the common honey bee and the sweat bees, belonging to the genera *Halictus* and *Andrena* are examples, carry the disease from one blossom to another. These flower-visiting insects are very efficient in transporting the disease, and other things being equal, the later a pear blooms the more complete its infection. If our pomaceous fruit blossoms continued to open during the summer, the destruction by pear blight would doubtless be almost complete. Ordinarily the closing of the blooming period terminates the multiplication and distribution of the disease.

Mr. M. A. Carleton discussed 'The Spread of Smut and Bunt in Wheat as affected by Dry Seasons and the Earliness of Varieties.' It has been pretty well known for some time that smut and bunt in cereals are much more prevalent in dry seasons and in dry regions. Experiments and observations made by the U. S. Department of Agriculture have also shown that these fungi, when attacking wheat particularly, are more likely to appear in early

varieties than in those that mature later. Thus it is very common for Japanese wheats to be infested with smut when introduced into this country, and Japanese varieties are always quite early in ripening. Now, as the tendency of dryness and heat is to produce early ripening of plants, it appears that there may be some relation between these parallel facts, and the question is a very interesting one as to why these conditions exist. As a rule the smut is propagated by germinating in the ground with the grain itself, infecting the young plant at that time and growing up through the plant as the plant grows, finally breaking out at the surface in the wheat head. One of two things therefore it seems may be true, either that the abnormal condition of the plant produced by its infection with the smut causes the plant to ripen earlier, or, on the other hand, that the early maturity of the plant allows the smut to work its way to the surface before the plant has grown entirely beyond it. Many observations seem to show that the latter is true, although it is by no means established. The tendency in dry seasons and in early ripening is always to produce more fruit or grain and less of the vegetative portion of the plant. As the smut finally produces its spores at the surface in the head, this condition would naturally favor the maturity of the smut. On the other hand, in later ripening sorts and in moister regions or seasons of greater moisture, the growth of the plant being more rapid and the maturity of the fruit occurring later, the plant is enabled in a sense to outgrow the development of the smut.

A portion of the evening was devoted to a symposium on 'Environment as a Factor in Natural Selection,' the discussion being led by Messrs. W. J. Spillman and H. J. Webber. In the discussion Professor Spillman stated that environment is not only a factor in natural selection; it is the whole of it. It is more than this, for it is a factor in variation. As stated, therefore, the subject covers the whole field of natural selection. It is probable that natural selection has been overworked, and particular attention is called to the fact that much, perhaps most, variation is neither useful nor harmful, and therefore not amenable to the influence of natural selection. If this is true a great deal of what we see in living organisms is not due to natural selection, but merely to fortuitous variation, perhaps to mutations, as De Vries would have us believe.

It is really change of environment that is important in natural selection. These changes are frequently favorable in that they remove a condition which made selection more strict. Examples of these are common in the case of organisms transplanted to a new habitat, where their natural enemies are absent. Under such conditions variations become permissible that were not possible under the old conditions, and what was before an unimportant species may assume a very important place in the economy of nature.

Mr. Webber stated that while the majority of variations induced directly by the influence of environment are not inherited; nevertheless, the influence of environment serves to destroy those individuals which do not vary in the direction of adapting themselves to the environment. It is only those individuals, therefore, which possess desirable variations that are able to produce seed for the next generation. The action of the environment in the next generation would be exactly the same, those plants only which vary in the direction of fitting themselves to the environment being able to survive and produce seed. In this way natural selection would eliminate such variations as were unfitted to the environment, so that only those plants best fitted would propagate. This action continued through several years would eventually result in rendering hereditary the characters fitted to the environment.

HERBERT J. WEBBER,
Corresponding Secretary.

DISCUSSION AND CORRESPONDENCE.

WHAT IS NATURE STUDY?

THERE seem to be many conflicting definitions in attempts to answer the above question. Here are two examples: "Nature study, as used in this paper, is understood to be the work in elementary science taught below the high school—in botany, zoology, physics, chemistry and geology. We should aim to define re-

sults. Gushing sentimentalism or mere rambling talks will be as barren in results as undigested statistics. To avoid this, the teacher should always have a definite plan before her when the lesson begins."—D. Lange, Supervisor of Nature Study, St. Paul, Minn.

"Nature Study is seeing the things which one looks at, and the drawing of proper conclusions from what one sees. Nature study is not the study of a science, as of botany, entomology, geology and the like. It is wholly informal and unsystematic, the same as the objects are which one sees. It is entirely divorced from definitions, or from explanations in books. * * * To-day it is a stone; to-morrow it is a twig, a bird, an insect, a leaf, a flower. * * * The problems of chemistry and of physics are for the most part unsuited to early lessons in nature study.

"If nature study were made a stated part of a curriculum, its purpose would be defeated."—L. H. Bailey, Cornell University, N. Y.

I have observed the different methods of teaching botany and zoology for many years past. So far as this country is concerned, I think what is now correctly termed nature study started with Louis Agassiz at Harvard, where he invariably set his special students in zoology to work on a starfish, a lobster, a clam or some other animal; not one specimen of one of these, but many of them, not alone those that were full grown, but those of all ages; not only dead specimens, but those that were alive, always with numerous comparisons. For months, the use of books was positively forbidden; and all that was told the student, excepting a few names of parts, was, 'You are right,' or 'You are wrong,' and if wrong, the student was kept at the work until he saw the thing right.

Agassiz was overflowing with enthusiasm. He would throw up both arms with exclamations of delight on seeing a specimen of a common shell-fish that was overgrown. This earnestness and enthusiasm helped secure faithful work from his students. Since working under Agassiz I have not had the slightest doubt that his method of studying nature or nature study was unsurpassed for advanced students. This method made a lasting impression on Harvard, on her presidents, her professors, and all the students who took his kind of work. Through these students of Agassiz and their students down to the third generation, this spirit of independent work has come filtering along for fifty years or more, till it has finally become widespread and deeply seated, and has recently burst forth into a great flame.

After the manner of Agassiz with his post-graduates, so the teacher of the grades below the high school will treat her young students, of course giving easier problems requiring but a little time each day. The teacher will show her interest, tact and enthusiasm to draw out the best work from her pupils. By all devices, she will seek to get the results of the combined observations of all members of the class before she lets them know her own views on the subject, and even then parts of the work may be left with pupils for further investigation.

With much that is good in nature study comes much that is positively injurious, and unfortunately large numbers are unable to distinguish between the true and the false. One writes a little book giving it some fancy title, distorts the drawings of some seeds and seedlings, inserting outlines of children's faces thereon; she writes some marvelous stories, and all these to help arouse and retain the interest of the child.

I have in my possession a neat drawing made by a student. He made two drawings to represent two honey bees just about to visit apple blossoms. The bees are not alike; each has two wings only; the heads and legs are unlike anything ever attached to bees. The apple blossoms are five-lobed (gamopetalous), with three stamens growing from the base of each lobe of the corolla. He has made drawings of imaginary insects seeking imaginary nectar from imaginary flowers. This student was trained in a state normal school. Such caricatures are absolutely worthless, in fact injurious, to any young person who makes them or even looks at them.

W. J. Beal.

Agricultural College, Mich.

ŒCOLOGY.

TO THE EDITOR OF SCIENCE: I share Professor Ganong's surprise that, after the word 'ecology' had been fully discussed in your columns by many leading naturalists (of whom Mr. Ganong was one), you should have admitted my belated remarks. I can only suppose that you recognized, what Mr. Ganong seems to have forgotten, that I am not responsible for the intervention of the Atlantic Ocean. Still I confess that I should not for the moment have forgotten the difference between the American and English languages. I can only say that if the spelling 'ecology' be not a vagary, the fact is to be regretted, since such contractions undoubtedly mislead those who wish to follow the excellent example of one of your correspondents and to use the Greek lexicon. I do not recognize the parallel with 'economy,' a word which came to us through the French, and which is a familiar everyday word, whereas 'œcology' is, and no doubt will long remain a purely technical term. I infer that here I have the support of Mr. Lester F. Ward.

As to the meaning of 'œcology,' I am glad to find myself in entire agreement with Mr. Ganong and Dr. Theodore Gill. But when the former belabors me for bringing a false accusation against botanists, in saying that they have restricted the meaning of the term, I must defend myself. I do not profess to speak with the authority of Mr. Ganong, whose studies in this branch of natural history we all admire; I speak merely as a casual skimmer of such publications as SCIENCE. It certainly appeared to me that the two authors whose papers suggested the recent discussion, namely Mr. H. S. Reed and Mr. H. C. Cowles, used the term as meaning 'œcological plant-geography.' The former entitles his paper 'The Ecology of a Glacial Lake'; does Mr. Ganong seriously maintain that this means 'The science of the adaptation of a glacial lake to its surroundings?' The latter (whatever he may have said 'in his elaborate paper' here distinctly asserted that the 'phytogeographic' was one of the two aspects presented by 'all ecological problems,' and his paper dealt solely with this aspect. Your own editorial explanation of the term laid even more stress on geographic distribution. Surprised at this, I consulted one or two botanical friends, who assured me that by 'œcology' they really did understand the study of plant-associations. I therefore turned to Mr. Robert Smith's paper in *Natural Science* and found that he did not use the term 'œcology' in the same sense as the botanists just alluded to, but used instead the phrase 'œcological plant geography,' which I quoted in my previous letter. Mr. Ganong need not have hunted up all the instances of the words 'œcological' and 'œcology' in Mr. Smith's paper. I admit that the latter does occur once (Mr. Ganong says 'four times'). But my whole point was that Mr. Smith used it with its full and correct meaning, and that he did not mention it as an equivalent for the subject of his paper.

I trust Mr. Ganong will now see that, though my ignorance of botanical literature may have led me to give too extended a form to my statement, still the use of the term in a restricted sense does actually obtain among botanists. Indeed I am assured by a botanical colleague that such use is increasing. I hope therefore that some of Mr. Ganong's hearty blows will have glanced off me on to the shoulders of the real offenders.

The whole object of a technical terminology is precision and unambiguity of language. The moment a term ceases to be used in the strict sense of its original proposer, this object is defeated.* The fact that there are signs of such a change in the case of the word 'œcology' justifies a protest before it is too late.

F. A. BATHER.

MASS AND WEIGHT.

TO THE EDITOR OF SCIENCE: I notice in your issue of June 13, a communication from Dr. Goodspeed, on the subject of 'Mass and Weight.' I am glad that attention is called

* Professor W. M. Wheeler uses 'Ethology' "in the place of the less satisfactory 'ecology'" (SCIENCE XV., p. 774, May 16, 1902). Why is 'ecology' less satisfactory, if not for this very reason?

to this subject, as I think that some reform is greatly needed. I agree with him in all that he says except that I do not think the term 'measurement' is the proper one to take the place of the common title 'weights and measures.' Under the latter title is always understood a list of the *units* and their equivalents, and therefore the term 'measurement' does not apply. In view of the fact that the units of weight are *measures* quite as well as the units of length are, it seems to me a much better title would be simply 'measures,' and I would urge the adoption of that title in place of the word 'measurement,' suggested by Dr. Goodspeed.

Carl Hering.

Philadelphia.

SHORTER ARTICLES.

DIVERGENCE OF LONG PLUMB-LINES AT THE TAMARACK MINE.

In September last two very long plumb-lines were hung in No. 5 shaft of the Tamarack Mine at Calumet, Michigan. They were 4,250 ft. in length, being longer than in any previously recorded instance. They were of No. 24 piano wire and the bobs were of cast iron, weighing fifty pounds each. Great care was taken that there should be no interference from projecting objects nor from dropping water, of which indeed there is not a great deal in the shaft. Measurements between the lines taken at surface and at their lower extremities showed a divergence to the amount of 0.11 ft. A divergence of 0.07 ft. remained after the western wire had been moved about 1.25 ft. further west to ensure its freedom from obstacles. Thinking that the air pipes which run down the western end of the shaft might, through magnetic action on the bob nearest them, be causing this divergence, I advised the use of lead bobs in a plumbing of No. 2 shaft, which took place a little later. Although the length of the lines in No. 2 was about 120 ft. less than when they hung in No. 5, and although the lead bobs were used, there was yet a divergence of 0.10 ft.

The publication about this time in the Houghton *Daily Mining Gazette* of the fact that a divergence had been observed attracted wide attention, and brought forth many attempts to explain its existence. Those immediately cognizant of the conditions had no satisfactory theory to offer. One of the explanations was that the divergence was due to the greater attraction of the material at the end of the shaft for the bob hanging nearest it. It is remarkable how many engineers and other trained persons held to this theory. There seems to exist a general lack of appreciation of the forces of gravitation, except in the single instance of the force between the earth and objects upon it. It is of course true that the attractions on either bob toward the ends of the shaft are different, the stronger being toward the end nearest to which it hangs. Furthermore, these differences of attraction tend to diverge the lines. Their amounts, however, are in this case so insignificant as to put them quite out of consideration in attempting to explain the divergence. Their sum is only a few hundredths of a grain, and the consequent divergence only about 0.001 ft.

Professor Hallock, of Columbia University, suggested the theory of repulsion between like poles at the lower extremities of the wire, but afterwards modified this to include repulsion between like consequent poles distributed along the wires.

Permission having been granted me to carry on further experiments in No. 4 shaft of the Tamarack Mine, there were hung in this shaft bronze wires No. 20 B. & S. gauge, carrying 60-pound lead bobs. These lines were approximately 15 ft. apart and 4,440 ft. in length. By a simple system of triangulation the distance between the mean positions of their lower extremities was determined, while the distance between them at surface was directly measured. It is thought that these distances were compared with an error not greater than 0.003 ft. A small convergence of 0.028 ft. was observed. The steel wires were then hung in the same position at the top, and the positions at the bottom observed, both with lead and with iron bobs. The bronze wires were hung a second time, but somewhat nearer together, and were found practically parallel. The steel lines showed a slight con-

vergence. Subsequently the bronze wires with lead bobs were hung in No. 5 shaft in the same position, as nearly as might be, as was occupied by the steel wires in September. The divergence was greater, amounting this time to 0.141 ft. The results are exhibited in the following table:

Date, 1902.	Shaft.	Wires.	Bobs.	Distances in feet.		Convergence—. Divergence+.
				Surface.	Lower Extremities.	
Jan. 3	No. 4	Bronze.	Lead.	15.089	15.061	−0.028
" 6	" 4	Steel.	Lead.	15.089	15.074	−0.015
" 6	" 4	Steel.	Iron.	15.089	15.062	−0.027
" 9	" 4	Bronze.	Lead.	14.607	14.611	+0.004
" 16	" 5	Bronze.	Lead.	16.709	16.850	+0.141

The data shown in the table seemed to afford ample experimental proof that neither gravitation nor magnetism could account for the divergence originally observed. Further, it seemed that the results pointed clearly to the currents of air in the shafts as the disturbing cause.

Until No. 5 shaft is connected with other portions of the mine its ventilation is accomplished by dividing it into two parts by means of a tight casing which sets off one compartment and the ladder-way, at the western end of the shaft, to serve as an air chimney for the up-cast draft. At the time of the September and January plumbings there existed at different levels a number of openings in this casing. The west wire hung in the air chimney, and these openings permitted a rush of air from the down-cast side into the up-cast portion, the effect of which would be to move the west wire toward the west and thus produce a divergence. To make the proof as complete as possible it was decided to hang the bronze wires once more in this shaft, but to hang the west one in the compartment next the air chimney, rather than in it. It seemed that if both wires were hung in the down-cast portion the divergence ought to disappear. Moreover, communication between the air chimney and the down-cast portion was carefully stopped off as far down as the extent of the wires, and, to further prevent circulation, the shaft was covered at the top as soon as the wires were in position. Since a considerable difference in temperature exists between the bottom of the shaft and the surface, it was not possible to stop all circulation. There remained a considerable convection circulation whose down-cast portion was concentrated along the casing above referred to. The measurements between the wires were, at surface 11.944 ft., at bottom 11.962 ft., showing a divergence of 0.018 ft. This divergence was easily accounted for by the convection current just described.

The difference between the divergence of the steel wires hung in this shaft in September, and of the bronze ones in January is explained by the fact that the circulation in the warmer weather of September was much less vigorous than in January and, further, that the steel wires afforded the smaller surface to be acted upon.

The question of air currents had been considered early in the experiments. That they could account for the divergence was very slowly admitted by the observers, inasmuch as it was difficult to believe that currents of air could be of the steadiness, in both volume and direction, which would be necessary to permit the constancy which was observed in the mean positions of the lines. The mean positions were observed on scales, placed close to the wire. Most of the time scales divided into sixteenth inches were used. For hours at a time the variations of the mean position of a wire would not exceed three or four tenths of a scale division. The mean position was determined by drawing the wire aside and allowing it to vibrate, as in determining the resting point of a balance by the method of vibrations.

The responsibility of the air currents once admitted, it was found by studying the conditions in No. 2 shaft that the divergence there observed could be satisfactorily explained. The shaft is down-cast and the air leaves it at the west end to reach the mine. The small convergence observed in No. 4 shaft can likewise be accounted for by the swirl of the currents as they enter this shaft, which is up-cast. The contour of the walls

of the plats is such that the currents of air hugging the outside of the curve as they enter the shaft will have a tendency from the west wall toward the center. Moreover, it appears that this tendency will be stronger close to the wall than a little distance away. When therefore on the 9th of January, the west wire was moved eastward, lessening the distance between the lines, the wires hung more nearly parallel than when this wire was close to the wall of the shaft.

It seems therefore that a very simple cause was at the bottom of the divergence. The remarkable fact is that the currents of air should be so constant in their action. When, however, the great depth of the shafts is considered, also the constancy for considerable periods of time of the temperatures which may influence these currents, it seems reasonable that this steadiness should exist.

F. W. McNair.

Michigan College of Mines,
Houghton, Mich.

SEX IN SEED PLANTS.

Probably everyone who has tried it will say that it is not easy to teach students the relation between pteridophytes and seed plants. Yet by following closely the origin of the sporophyte and its gradual evolution the subject can be made clear if all conditions are favorable. One important condition is that the text-books consulted by the student shall be perfectly clear, that there shall be no confusion of terms.

In popular accounts of plants, as in popular works on science generally, one must expect to find technical subjects treated in rather offhand fashion. But in works planned for college students it does not seem unreasonable to ask for simple accuracy. Now it has long been known that, among the seed plants, 'the plant' is the sporophyte, a non-sexual organism. The stamens therefore cannot be male organs nor the carpels female organs. Placing the pollen upon the stigma is not fertilization and every botanist knows it. There are no such things as male and female flowers, nor flowers which are unisexual or hermaphrodite.

Notwithstanding these well known facts, many botanists continue to use these inaccurate expressions. Practically all of the European botanical journals are serious offenders. In our own country the first class journals use the modern terminology but many of the most widely used text-books do not. The most recently issued American text-book, a work intended for university students, contains the misleading and irrational terms mentioned above.

Methods of teaching botany are frequently discussed at educational conventions. To the writer it seems that what we need is not some new and fancy method of teaching but a knowledge of facts by the teacher and an ability to select a text-book which is clear and accurate in its terminology—not muddled and confused.

Francis Ramaley.
University of Colorado.

HARVARD COLLEGE OBSERVATORY ASTRONOMICAL BULLETIN.

The determination of the law governing the variation in light of the planet Eros (433) is one of the most interesting problems in Astronomical Photometry. A similar variation in light of the planets Sirona (116) and Tercidina (345) has been announced by Dr. M. Wolf, of Heidelberg. Both objects are favorably situated for observation this summer. The opposition of Sirona occurs on June 15, 1902, Magn. 10.9. Accordingly the following ephemeris for Greenwich Midnight has been computed by Mr. F. E. Seagrave, of Providence, R. I., from the elements given in the Berlin Jahrbuch for 1904.

EPHEMERIS.

1902.	J. D.	R. A.	Dec.	Log r	Log Δ
		h. m. s.	° ′ ″		
May 26.5	241 5896	17 53 45.8	−24 54 30	0.45135	0.27197
June 5.5	5906	17 45 10.2	−25 5 25	0.45349	0.26518
June 15.5	5916	17 35 36.5	−25 12 39	0.45560	0.26468
June 25.5	5926	17 26 7.8	−25 15 52	0.45767	0.27063
July 5.5	5936	17 17 43.2	−25 15 49	0.45969	0.28260

Photographs taken at Cambridge on June 5 and June 8, 1902, with the 8″ Draper Telescope, indicate a correction to this ephemeris in R. A. of +0.1m, and in Dec. of —1′. Photographic enlargements of this region will be furnished to observers who will undertake the required observations.

The opposition of Tercidina occurs on August 3, 1902, magn. 11.5, in R. A. 20h 50.4m, Dec. —0° 40′. Daily motion in R. A. —0.9m, in Dec. —4′.

EDWARD C. PICKERING.

A GRADUATE SCHOOL OF AGRICULTURE.

THE first session of a graduate school of agriculture held under the auspices of the Ohio State University, and with the cooperation of the United States Department of Agriculture and the Association of American Agricultural Colleges and Experiment Stations, will open at Columbus on July 7 and will continue for four weeks. The purpose of the school is to give advanced instruction in the science of agriculture, and particularly in the methods of investigating agricultural problems and teaching agricultural subjects. Only persons who have completed a college course and taken a bachelor's degree, or who are recommended by the faculties of the colleges with which they are associated, will be admitted to the privileges of the school. Instruction will be given in four courses—agronomy, zootechny, dairying, and animal and plant breeding. The courses in these subjects will run parallel; except that the course in breeding will be so arranged that it can be taken by students in any of the other courses. The Saturday morning periods will be devoted to lectures and conferences on agricultural pedagogy and special topics of general interest. The equipment of modern dairy apparatus and machinery and apparatus for instruction in soil physics is especially complete. Some of the apparatus used in the investigations of the Bureau of Soils of the U. S. Department of Agriculture will be transferred to Columbus for the use of the school. This bureau is now conducting a soil survey of the region in the immediate vicinity of Columbus, and the students of the school will have an opportunity to observe the field methods of this survey.

The breeders of Ohio will contribute live stock for judging and demonstration purposes in connection with the courses in zootechny and animal breeding. An especially selected library of works on agriculture and agricultural science will be provided.

Dr. A. C. True, chief of the Division of Agricultural Colleges and Experiment Stations of the Department of Agriculture, is dean of the school. The faculty will consist of about thirty instructors, including the heads of the agricultural departments of state universities and agricultural colleges and the directors and other officers of experiment stations in different parts of the country, as well as chiefs of bureaus and other officers of the U. S. Department of Agriculture.

SCIENTIFIC APPOINTMENTS UNDER THE GOVERNMENT.

SEVERAL positions in the scientific departments of the government will be filled as the result of civil service examinations in July.

On July 10 an examination will be held to fill three vacancies in the position of laboratory assistant in the National Bureau of Standards, at a salary of $900, $1,000 and $1,400 per annum, and to other similar vacancies as they may occur.

The examination will consist of the subjects mentioned below, which will be weighted as follows:

Education and training, including training in mathematics and mathematical physics. (State all courses in these subjects taken in college or later.)	20
Experience, including (*a*) laboratory work in electricity and general physics done in college or later; (*b*) any other experimental work or original research; (*c*) other experience likely to be helpful in the position of laboratory assistant	30
One or more of the following optional subjects: (*a*) Theoretical and applied electricity and electrical testing; (*b*) Theoretical and experimental optics; (*c*) Mechanics of solids and fluids with applications to the testing of weights and measures	50

Competitors will be assembled only for the tests under the third subject. Three hours will be allowed for subject *a*, and two hours each for subjects *b* and *c*. Applicants must show that they have been graduated from colleges or technical schools, or show that they have obtained an equivalent scientific training. The Department desires that the appointee be not less than 20 nor more than 40 years of age and be in good physical condition. A preliminary rating will be made on the first two subjects as shown by the application and accompanying vouchers, and those applicants who fail to attain at least 70 per cent. on this portion of the examination will not be given the tests under the third subject.

On July 8 and 9 examinations will be held to fill at least four vacancies as computers in the Coast and Geodetic Survey at a salary of $1,000 a year. The subjects and weights are:

Algebra	10
Plane and solid geometry	10
Trigonometry	15
Elements of calculus	15
Practical computations	50

On July 11 and 12 an examination will be held to fill twelve vacancies in the position of aid in the Coast and Geodetic Survey, at a salary of $720. The age limit is eighteen to twenty-five years and the subjects and weights are:

Mathematics, including the elements of calculus	10
Practical computations	10
Astronomy	10
Physics	10
Surveying	10
Modern languages, translation from one European language	10
Drawing and descriptive geometry	10
Training and experience	10
Physical examination	20

On July 15 there will be an examination to fill the position of assistant in the Road-material Laboratory, Division of Chemistry, Department of Agriculture, with a salary of $600 with prospects of promotion; and on July 15 and 16 there will be an examination for assistant (piece-work computer) in the Naval Observatory and the Nautical Almanac Office.

THE PITTSBURGH MEETING OF THE AMERICAN ASSOCIATION.

Arrangements have now been made so that certificates will be honored from points in the territory of the Western Passenger Association which show the purchase of tickets for the Pittsburgh meeting of the American Association on June 26–30 inclusive in addition to June 19–25 inclusive.

George A. Wardlaw.

SCIENTIFIC NOTES AND NEWS.

Sir Joseph Dalton Hooker has been appointed a foreign knight of the Prussian Ordre Pour le Merite for Science and Arts. Sir Joseph Hooker, who was director of the Kew Botanical Gardens from 1865–1885 and president of the Royal Society from 1872–1877, will celebrate his eighty-fifth birthday on June 30.

The Huxley Lecture at Charing Cross Hospital will be delivered this year by Dr. William H. Welch, of the Johns Hopkins University.

Lord Rayleigh has been elected a corresponding member of the Vienna Academy of Sciences.

Dr. S. Alfred Mitchell, tutor in astronomy at Columbia University, has been elected a fellow of the Royal Astronomical Society of England.

The officers of the American Medical Association elected at the Saratoga meeting are: *President,* Frank Billings, of Chicago; *Vice-Presidents,* W. A. Witherspoon, of Tennessee, G. F. Comstock, of New York, C. R. Holmes, of Ohio, James H. Dunn, of Minnesota; *Secretary,* G. H. Simmons, of Illinois; *Treasurer,* H. P. Newman, of Illinois; *Orators:* Medicine, J. M. Anders, of Philadelphia; Surgery, A. F. Jones, of Omaha; State Medicine, W. H. Welch, of Baltimore. Little Rock, Arkansas, was recommended for the next place of meeting.

The North Carolina Academy of Science was founded at Raleigh, on March 21, 1902. The following officers were elected for the current year: *President,* W. L. Poteat, Wake Forest College; *Vice-President,* T. Gilbert Pearson, State Normal College; *Secretary and Treas-*

urer, Franklin Sherman, Jr., State Entomologist; *Executive Committee,* W. L. Poteat, Franklin Sherman, W. W. Ashe, H. H. Brimley, Tait Butler, J. L. Kesler, B. W. Kilgore, F. L. Stevens and H. V. Wilson. The first meeting for the presentation of papers will be held during the first week in October.

OXFORD UNIVERSITY will on June 24 confer its honorary degree of D.C.L. on William H. M. Christie, C.B., F.R.S., Astronomer Royal, and on Arthur W. Rücker, F.R.S., principal of the University of London. The degree will also be conferred on Mr. Choate, Ambassador of the United States.

PRINCETON UNIVERSITY has conferred the degree of LL.D. on Mr. Morris K. Jesup, president of the American Museum of Natural History.

PRESIDENT NICHOLAS MURRAY BUTLER, of Columbia University, has received the degree of doctor of laws from Princeton University. It is understood that he will receive the same degree from Yale University and from the University of Pennsylvania. He will make the commencement address at the latter university.

COLUMBIA UNIVERSITY has conferred the degree of D.Sc. on Professor S. B. Christie, who has held the chair of mining and metallurgy in the University of California since 1885.

M. ÉMIL LAURENT, professor of agriculture in the Belgian national school at Gembloux, has been elected a correspondent of the Paris Academy of Sciences.

DURHAM UNIVERSITY has conferred the degree of D.C.L. on Sir W. S. Church, president of the Royal College of Physicians, London, and on Dr. Thomas Annandale, professor of clinical surgery at Edinburgh.

PROFESSOR CH. RICHET, the eminent French physiologist, has resigned the editorship of the *Revue Scientifique,* which he has held for twenty-five years. He is succeeded by M. Héricourt.

UNIVERSITY AND EDUCATIONAL NEWS.

DR. FRANCIS L. PATTON resigned the presidency of Princeton University on June 9, but retains the Stuart professorship of ethics and the philosophy of religion. Dr. Woodrow Wilson, McCormick professor of jurisprudence and politics, was elected as his successor.

AT the commencement exercises at the Johns Hopkins University last week, President Remsen announced that over $900,000 had been subscribed toward the million dollar endowment fund.

THE University of Pensylvania has received a gift of $100,000 for the construction of the new medical buildings, the name of the donor being withheld for the present.

MR. A. A. POPE, of Cleveland, has given the Western Reserve University $100,000 for buildings.

PRESIDENT JOSEPH SWAIN, of Indiana University, accepted the presidency of Swarthmore College, on condition that the endowment be increased by $400,000. This sum has now been subscribed.

WELLS COLLEGE has received $50,000 from Mr. Henry A. Morgan and $25,000 from Mr. N. L. Zabriskie for buildings and equipment.

IT is announced that the donors of the new physical laboratory for Wesleyan University, which is to cost $75,000, are Mr. Charles Scott, Sr., Philadelphia, a trustee of the university, and Mr. Charles Scott, Jr., New York, a graduate of the class of '86. The building is to be a memorial of John Bell Scott of the class of '81.

THE appeal by the governors of University College, Dundee, has brought in over £24,000.

DR. E. H. GRIFFITHS, principal of University College, Cardiff, and formerly lecturer on physics at Cambridge, has offered to give his collection of apparatus to the college, if a building for a laboratory is provided at a cost of £2,000. He suggests that the laboratory be named in honor of the late Viriamu Jones. If Cardiff does not accept the offer, the collection will be presented to the National Physical Laboratory at Teddington.

THE Sir John Cass Technical Institute, London, erected at a cost of over $150,000, was formally opened on May 5 by Lord Avebury.

The draft charter of the proposed University for Liverpool, settled by the council of University College, gives powers as to the conferment of degrees in all the recognized faculties, as well as in the faculty of commerce, which embraces the sciences of economics, geography, banking and commercial law. It also gives powers to admit new constituent colleges; to recognize halls of residence for students; to establish new professorships and lectureships; and provides for the establishment of external examiners.

The following promotions have been made at the Johns Hopkins University:—George B. Shattuck, Ph.D., now associate, to be associate professor of physiographic geology; Caswell Grave, Ph.D., now assistant, to be associate in zoology; Louis A. Parsons, Ph.D., to be assistant in physics; William G. MacCallum, M.D., now associate, to be associate professor of pathology; Guy L. Hunner, M.D., now instructor, to be associate in gynæcology; Walter Baumgarten, M.D., to be assistant in medicine; Florence R. Sabin, M.D., to be assistant in anatomy; Benjamin R. Schenck, M.D., to be instructor in gynæcology.

Dr. H. C. Warren has been promoted to a full professorship of experimental psychology at Princeton University.

Mr. George F. Gebhardt, instructor in the Armour Institute of Technology, at Chicago, has been elected to the chair of mechanical engineering.

The following appointments and changes in the scientific faculty of the University of North Carolina were announced at the recent commencement of that institution: W. C. Coker, Ph.D. (Johns Hopkins), now at Bonn, associate professor of botany; J. E. Duerden, Ph.D. (Johns Hopkins), acting-professor of biology during the leave of Dr. H. V. Wilson, head of the department; Ivey F. Lewis, A.B. (U.N.C., 1902), assistant in biology; M. H. Stacy, Ph.B. (U.N.C., 1902), instructor in mathematics; Archibald Henderson, Ph.D. (U.N.C.), associate professor of mathematics, was given a year's leave of absence, during which period he will fill an instructorship at the University of Chicago; George P. Stevens, A.B. (U.N.C., 1902), assistant in mathematics; J. E. Latta, A.M. (U.N.C.), instructor in physics, was given a year's leave of absence. He has accepted a scholarship at Harvard; H. R. McFadyen was appointed assistant in physics; Hazel Holland, assistant in chemistry, and W. M. Perry, assistant in pharmacy.

Dr. Cady Staley, since 1886 president of the Case School of Applied Science, has resigned.

Mr. John W. Abercrombie has been elected president of the University of Alabama. The Rev. Dr. Guy P. Benton, president of the Upper University of Iowa, has been elected president of Miami University.

Dr. James H. Carlisle has resigned the presidency of Wofford College, at Spartanburg, S. C., and Professor Henry Nelson Snyder, professor of English, succeeds him. Dr. Carlisle, who is seventy-seven years old, has been elected president emeritus and professor of astronomy and ethics.

Dr. Thomas Ruggles Pynchon, since 1877 professor of moral philosophy at Trinity College, has resigned and has been made professor emeritus. Dr. Pynchon was president of Trinity College from 1874 to 1877 and was from 1854 to 1877 professor of chemistry and natural sciences.

Mr. Graham Balfour, M.A., assistant secretary of the Oxford Examinations Delegacy, has been appointed director of technical instruction under the Staffordshire County Council, in succession to Professor Thomas Turner, recently elected to the chair of metallurgy at Birmingham.

Mr. Douglas A. Gilchrist, B.Sc., professor of agriculture and director of the agricultural department at the college, Reading, has been appointed professor of agriculture at the Durham College of Science, Newcastle, in succession to Professor T. H. Middleton, M.Sc., who was recently elected to the chair of agriculture in the University of Cambridge.

Mr. Herbert Stanley Jevons, son of the late Professor Stanley Jevons, has been appointed to a lectureship in mineralogy at the University of Sydney, New South Wales.